

ESSAYS ON ECONOMICS AND EDUCATION

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ABSTRACT

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This dissertation broadly focuses on how to improve equity in education. The first chapter focuses on education at the primary level and analyzes whether progressive vouchers in education can serve as a tool to decrease socioeconomic stratification at the school level and increase educational outcomes for low-income students. I use the Chilean setting, where a universal voucher system has been in place for over three decades, and analyze the impact of a major reform where voucher amounts were increased by 50 percent for students in the lowest 40 percent of the income distribution. Progressive vouchers were implemented in Chile to help low-income students benefit from school choice; increasing the revenues that schools receive for serving low-income students and lowering the relative prices of private voucher schools for eligible parents. I use a national dataset to implement a regression discontinuity design exploiting that eligibility is a discontinuous function of a socioeconomic ranking. Results reject that eligible students chose schools with higher test scores or average SES, and that they are doing better than non-eligible students in math and language test scores. Findings, I argue, are partly a consequence of the multiple barriers that low-income students face when choosing a school, including lack of information, the complexity associated with evaluating a substantial number of options, and issues of social belonging that prevent them from attending better performing schools.

The second chapter focuses on education at the tertiary level and analyses whether loans for higher education can help to increase tertiary education for low-income low-performing students. I use data from Chile and exploit the fact that access to loans for universities and technical institutions is a discontinuous function of students' academic performance. The latter allows me to implement a regression discontinuity design to look at the causal impact of different types of loans on higher education access, persistence and graduation. Results

show that loans for universities induce low-performing students away from technical institutions and towards higher quality university alternatives, where they have little chances of succeeding. This increases the total amount of time and money that students spend without substantially increasing, or even decreasing, their graduation rates and expected incomes. Loans for technical institutions are better in that they keep students away from alternatives that are too expensive or academically demanding. Results point to the unintended costs of offering university loans to low-performing students, stemming from a potential mismatch between low-performing students and higher quality university alternatives.

The third chapter, joint with Juan Matta, analyzes the role of social interaction in higher education choices. In particular, we analyze spillovers from older to younger siblings in the choice of college and major. We use data from Chile and exploit discontinuous admission rules generated by Chile's centralized system of admission to postsecondary education. Our findings reveal strong sibling spillovers in the choice of major/institutions. Having an older sibling enrolling in a given major within an institution, as opposed to just applying, increases by 87% the likelihood of enrolling in that same major/institution combination, and it increases by 51% the probability of enrolling in any major within that same institution. An analysis of potential mechanisms suggests that spillovers are present even when siblings are far apart in age and are unlikely to attend college together, and even in cases where they are likely to be well informed about the program. Results provide an explanation as to why low-income students may be underrepresented in some high quality educational alternatives.

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Dedication

To Javier and Lucía, for all their love and support.

Chapter 1

How can Progressive Vouchers Help the Poor Benefit from School Choice? Evidence from the Chilean Voucher System

I Introduction

Advocates of school vouchers typically argue that these help promote consumer choice, personal advancement, and competition. From their perspective, they can lower educational costs even as they increase school quality.¹ Despite this expectation, there is still no consensus on whether voucher programs improve average outcomes. Moreover, there is concern that they can generate a sorting of students across schools along characteristics like income and ability, which possibly leads to lower educational outcomes for less-advantaged students (see [Manski, 1992](#); [Epple and Romano, 1998](#); and [MacLeod and Urquiola, 2012, 2015](#) for theoretical work; and [Chakrabarti, 2009](#); [Hsieh and Urquiola, 2003, 2006](#); [McEwan et al., 2008](#); and [Muralidharan and Sundararaman, 2015](#) for empirical evidence).

A question that remains unanswered is whether a more careful voucher design could preserve the potential efficiency benefits from competition while mitigating socioeconomic stratification. For instance, several authors have suggested that deviating from a flat voucher to one that conditions the subsidy on student characteristics like income could ameliorate sorting impacts and help low-income students benefit from voucher systems ([Epple and Romano, 2008](#); [Nechyba, 2000, 2003](#)). The underlying idea is that differentiated vouchers would give schools an incentive to serve low-income students, which mitigates the temptation to admit only students with relatively high socioeconomic status or ability. Despite much discussion, however, there is still little rigorous empirical evidence on actual impact of such differentiated vouchers.

In this paper I look at a major educational reform implemented in Chile, a country that has used vouchers for over three decades. Specifically, in 2008 the voucher amount was increased by 50% for students in the lowest 40% of the income distribution. A number of papers have studied this change, but while most of them, with the exception of [Feigenberg et al. \(2017\)](#), have found a positive effect on educational outcomes ([MINEDUC, 2012](#); [Correa et al., 2014](#); [Villarroel, 2012](#); [Mizala and Torche, 2013](#); [Neilson, 2013](#); [Navarro-Palau,](#)

¹see [Hoxby \(2003\)](#) for a review of how school choice might affect school productivity.

2017), there is an ongoing discussion on the potential mechanisms through which progressive vouchers could have helped improve the educational outcomes of the poor.

In part the lack of agreement on the potential mechanisms reflects an identification challenge. While several researchers have noted that it would be natural to assess the program using a regression discontinuity design, this has not been feasible due to a lack of information. In this paper I use a new matched administrative dataset to implement such an approach and to explore the direct effect that differentiated vouchers had on their beneficiaries.

Chile is one of the few countries with a universal voucher system, where both public and private voucher schools get paid a voucher amount for each student. Progressive vouchers were implemented in Chile to acknowledge the fact that the costs of educating low-income students are higher and also with the aim of giving low-income parents access to a sub-group of private voucher schools that charge add-ons to parents and that have on average higher test scores than public schools and private voucher schools that do not charge add-ons to parents. Because of the latter, in order to receive the extra resources from targeted vouchers, schools had to sign up for the policy and agree, among other things, not to charge add-ons to low-income parents, although they could still charge add-ons to high-income parents.

There are several mechanisms through which the reform could have increased educational outcomes for eligible students. First, public schools and private schools that chose to join the policy received additional revenue for each eligible student that could be used to improve educational results. Importantly, the law entailed that money from targeted vouchers had to be spend on educational improvement plans aimed to increase the educational results of eligible students. Second, the higher revenues provided schools an incentive to compete for the enrollment of low-income students. This could have led schools to increase their overall quality or the quality that they provide to low-income students. Third, because eligible parents no longer had to pay add-ons to attend private voucher schools and because schools had higher incentives to admit these students, the policy could have expanded choice sets for

eligible parents. This could have led to better educational outcomes for students if eligible parents were now able to choose better schools.

Nevertheless, previous mechanisms could also prove to be ineffective. If, regardless of the voucher increase, higher performing schools are unwilling to serve low-income students, then they may have abstained from participating in the policy, limiting parents' choices. Alternatively, if parents face barriers aside from price —such as distance or lack of information— that prevent them from attending better performing schools, then the policy might have been unable to change students' distribution across schools. Also, if schools have local market power, it is possible that the policy simply led to increased revenues for existing schools without any impact on educational outcomes for eligible students.

In this paper, I exploit the fact that eligibility for progressive vouchers is a discontinuous function of a socioeconomic ranking to implement a regression discontinuity design. This allows me to estimate the causal impact of being eligible for a targeted voucher on the school choices and educational outcomes of students who entered 1st grade in 2012, four years after the program was first implemented. I find that being eligible for a targeted voucher had no impact on the probability of choosing a private school, the test scores of the chosen school, the socioeconomic status of the chosen school, the average class size of the chosen school, or the distance traveled to school. The results do show that eligible students chose schools that charge higher add-ons to non-eligible parents, but this effect is small in magnitude, with eligible students choosing schools that charge approximately 3USD more to non-eligible parents. In terms of educational outcomes, results show no impact of being eligible for a targeted voucher on students' performance on a standardized language test that is given to students in second grade, and on a standardized language and math test that is given to students in fourth grade. Importantly, I am able to reject a positive impact above 0.04 standard deviations on each of these test scores.

A first contribution of this paper is to show that being eligible for a targeted voucher had no impact on parents' school choices. I argue this is driven by both demand and supply side

mechanisms. On the supply side, I am able to show that even though all public schools and roughly all private voucher schools that charge no add-ons to parents joined the policy, only 50% of private voucher schools that charge add-ons chose to join. Private voucher schools that chose to participate have lower prices, test scores, and socioeconomic status, compared to schools that chose not to participate. This result is in line with that of [Abdulkadiroglu et al. \(2015\)](#) who find that low-quality private voucher schools tended to select themselves into the Louisiana Scholarship Program. These results suggest that, despite the voucher increase, schools with the highest test scores were unwilling to participate in the policy and serve low-income students.

On the demand side, I am able to show suggestive evidence indicating that there are other barriers, aside from price, that prevent parents from attending higher test score schools. Even though private voucher schools with the highest test scores abstained from participating in the policy, there was still a substantial number of above average test score schools that charge add-ons and chose to join the program. These schools were now free for eligible parents and could have represented an improvement over public schools and private voucher schools that charge no add-ons to parents. Importantly, the extra voucher amount was typically higher than the add-ons charged by these schools, indicating that they might have had a special incentive to admit eligible as opposed to non-eligible students. I perform three exercises aimed at better understanding what barriers could be preventing parents from responding to this price decrease. I look at heterogeneous effect by mothers' education, heterogeneous effects by distance to the nearest private voucher school that charges add-ons to parents and joined the program, and I extend results further away from the discontinuity.

The effects could vary by mothers' education either because higher socioeconomic status parents might have a higher preference for school quality (see [Hastings and Weinstein, 2008](#) and [Bayer et al., 2007](#) for evidence on this), or because schools might choose to serve, among eligible students, those of higher socioeconomic status. In practice, however, I find no evidence of a differential impact on students whose mothers' have less than high school

education, or more than high school education.

Results could also vary by students' distance to a private voucher school that charges add-ons to parents and joined the policy, as distance has been found to be a major determinant of school choice ([Hastings and Weinstein, 2008](#); [Bayer et al., 2007](#)). However, when looking at the impact of the policy on students who live relatively close to a private voucher school that charges add-ons to parents and joined the policy, I find no effect on parents' school choices or students' educational outcomes.

Finally, I look at whether results look any different for those students who are further away from the discontinuity. A possible concern is that, because eligibility for targeted vouchers is determined on a yearly basis, eligible parents close to the cutoff might be afraid of losing their benefit and ineligible parents close to the cutoff might be expecting to gain the benefit. To address this issue I follow [Angrist and Rokkanen \(2015\)](#) and I use a matching strategy to estimate the impact of the program on a wider sample of students, comparing eligible and ineligible students who are further away from the discontinuity. This allows me to use a broader treatment and control group that is unlikely to change status from one year to the next. It also allows me to determine what the impact of being eligible is for students who are further away from the cutoff and therefore have lower or higher SES. Results from this analysis confirm previous findings and indicate that being eligible for a targeted voucher had no impact on the characteristics of the schools chosen or educational outcomes for students who are further away from the discontinuity.

These results indicate that, with a voucher system already in place, progressive vouchers were ineffective in terms of changing students' distribution across schools. This could be a result of students who are typically left behind in public schools or bad performing private voucher schools facing additional barriers that prevent them from attending higher test score schools. Barriers could include lack of information, the complexity associated with evaluating a substantial number of options, or issues of social belonging that lead them to choose schools where their own social group is majority. This result adds to an increasing

literature looking at behavioral barriers that prevent individuals from making what could be optimal educational choices (Radford, 2013; Pallais, 2015a; Smith et al., 2015a; Thaler and Mullainathan, 2008; Ross et al., 2013).

A second contribution of this paper is to show that targeted vouchers did not have a direct impact on the educational outcomes of their beneficiaries. There is no evidence that increased revenues or increased competition improved educational results for eligible as opposed to non-eligible students in the short or medium term. This could be reflecting that educational outcomes did not improve for low-income students as a result of this policy; or that results did improve, but that the benefits were captured by all students who are similar in terms of wealth, regardless of whether they were or not eligible.

This paper provides a thorough analysis of the mechanisms through which progressive vouchers could have helped to increase the educational outcomes of the poor in Chile, thus contributing to the ongoing empirical discussion on the effects of the 2008 reform. The results are also highly relevant from a public policy perspective. As school vouchers continue to be implemented across the world, and as the US moves towards increasing school choice, much can be learned from the Chilean voucher experience. This paper in particular helps to document that progressive vouchers in Chile did not lead to a resorting of students across schools, nor did they lead to an increase in educational outcomes for eligible as opposed to non-eligible students.

II Related Literature

Results from this paper relate to several strands of the literature. First, they relate to the literature looking at the impact of the 2008 Chilean reform on school choices and educational outcomes. The period from 2008 to 2012 saw an increase in test score results for low-income students in Chile that has been attributed to the 2008 reform by many.² Existing studies

²Figure 1.A.1 and Table 1.A.1 in Appendix 1.A perform a simple difference in difference analysis and shows that by 2012 the gap between eligible and ineligible students had decreased by roughly 0.08 standard

include [MINEDUC \(2012\)](#), [Correa et al. \(2014\)](#), [Villarroel \(2012\)](#), and [Mizala and Torche \(2013\)](#), all of which compare the academic outcomes of schools that chose to join the policy to the academic outcomes of schools that chose not to join and find a positive impact on test scores that ranges between 0.08 and 0.2 standard deviations. [Neilson \(2013\)](#), instead, compares the academic outcomes of low and high-income students and finds that targeted vouchers raised test scores for low-income students in 0.2 standard deviations. [Navarro-Palau \(2017\)](#), instead, uses variation from date of birth enrollment cutoffs to compare the outcomes of students who had a different exposure to the reform, and finds a significant though more modest effect of the reform on school achievement.

An exception is a more recent paper by [Feigenberg et al. \(2017\)](#) that argues that changes in parental education and household income could account for much of the decline in the achievement differential between low and high socioeconomic status students observed in the data. According to the authors, there is little evidence that the reform had a substantial effect on school inputs or that it altered the education market in a manner that could have raised achievement for low-income students.

Now, although most studies, aside from [Feigenberg et al. \(2017\)](#), agree in that the policy improved educational outcomes for poor students, there is an ongoing discussion about the mechanisms that could be driving these results. Studies that explicitly look at mechanisms include [Neilson \(2013\)](#) and [Navarro-Palau \(2017\)](#). [Neilson \(2013\)](#), on the one hand, uses a structural model of school demand and supply to constructs counter-factual simulations and isolate the different mechanisms through which the policy could have affected outcomes; concluding that approximately one third of the observed improvement is due to eligible families being able to choose better schools with the larger voucher, and two thirds of it is due to the rise in quality of existing schools in response to the policy. A major concern though, is that the structural model requires making important assumptions about school supply and demand, including, for example, that students can attend any school they are

deviations.

willing to travel and pay for, and that schools have no capacity constraints. To the extent that these assumptions are flawed, demand estimates could be biased and we could be over or under estimating the extent to which families responded to the policy by resorting across schools.

[Navarro-Palau \(2017\)](#), on the other hand, exploits variation from date of birth enrollment cutoff to compare the choices and educational outcomes of students who entered school right before or right after the policy was implemented. The author finds that the policy slightly decreased the probability that students attended public schools and that it increased the probability that they attended private schools with better average characteristics. However, there is no evidence of a positive effect on test scores for students more likely to switch schools. Based on these results, and an observed increase in test scores for students most likely to stay in public schools, the author concludes that the effects of the policy on test scores were caused by a response from public schools. A major concern with previous results, however, is that date of birth is not truly random and that the existence of pre-trends in enrollment and educational outcomes could be biasing the results.³

This paper contributes to the discussion by providing a new identification strategy to look at how the policy affected choices and educational outcomes for eligible students. The regression discontinuity approach represents a more reliable approach to look at the direct impact that the policy had on its beneficiaries. Results allow me to discard that differentiated vouchers changed students' distribution across schools and that the program improved the educational outcomes of eligible as opposed to non-eligible students.

Second, this paper relates to the existing empirical literature on school vouchers and student stratification. Among the existing papers are [Hsieh and Urquiola \(2003, 2006\)](#) and [McEwan et al. \(2008\)](#) both of which find that the growth of the private sector in Chile increased stratification by socioeconomic status. Some evidence of sorting has also been found in Sweden ([Sandström and Bergström, 2005](#); [Böhlmark and Lindahl, 2007](#); [Böhlmark](#)

³The author tries to address this by using RD estimates for pre-policy cohorts to account for biases. However, these trends could have changed over time biasing the results.

et al., 2015). In India, [Muralidharan and Sundararaman \(2015\)](#) find that students from lower casts were less likely to accept vouchers if awarded. This paper contributes to previous studies by showing that introducing a targeted voucher may be ineffective in terms of reducing students' stratification by socioeconomic status.

Third, the paper also relates to the existing empirical literature looking at the impact of school vouchers on educational outcomes. From a theoretical point of view, school vouchers can affect educational outcomes by allowing students to migrate from public to private schools. Also, when a program is large in scale, vouchers can affect educational outcomes through a re-sorting of students across schools and a potential increase in competitive pressure among schools.

In general, evidence for small-scale voucher programs is mixed. In the US, studies have typically found no effect of receiving a voucher on test scores for non-African American students and some evidence, albeit not very robust, of a positive effect for African American students. In contrast, there is more robust evidence that voucher programs had a positive impact on graduation probabilities, particularly for African American students.⁴ Worth mentioning is a more recent study by [Abdulkadiroglu et al. \(2015\)](#) that looks at the Louisiana Scholarship program and finds that vouchers reduced academic achievement for students, a result that the authors attribute to a selection of low-quality schools into the program.

Results for other countries are more positive than those for the U.S. In Colombia, for instance, studies find a positive impact of vouchers on test scores, as well as other long-term outcomes such as the probability of completing secondary school ([Angrist et al., 2002, 2006](#)).

Less empirical evidence can be found on the effects of large-scale voucher programs. Worth mentioning is an experimental study by [Muralidharan and Sundararaman \(2015\)](#) in India, where a large-scale voucher program was implemented. The authors find that four years after treatment, voucher lottery winners did not have higher test scores in Math,

⁴see [Wolf et al. \(2010\)](#) for evidence on the D.C. Opportunity Scholarship Program. [Peterson et al. \(2003\)](#); [Mayer et al. \(2002\)](#); [Chingos and Peterson \(2015\)](#) for evidence on the programs from the School Choice Scholarship Foundation implemented in New York City, Dayton, and Washington, D.C., and [Rouse \(1998\)](#); [Witte et al. \(2012\)](#) for evidence on the Milwaukee program

English, Science and Social Studies, although they did perform better in Hindi. However, the authors emphasize that private schools in India spend much less than public schools, implying a higher productivity of private schools. Interestingly, the authors find no evidence of spillover effects on public school students who do not apply for the voucher, or on private school students. Non-experimental studies looking at large-scale voucher programs, instead, typically do find positive effects of vouchers on public school students (see, among others, [Figlio and Rouse, 2006](#); [Gallego, 2013](#); [Chakrabarti, 2008](#); [Rouse et al., 2013](#); [Figlio and Hart, 2014](#)).⁵

The analysis I perform in this paper is somewhat different from that of previous studies as it focuses on the impact of increasing the voucher amount as opposed to giving students access to vouchers. Still, results contribute to the general literature by showing that increasing vouchers for low-income students and decreasing the prices that these students have to pay to attend private voucher schools, will not necessarily lead them to attend higher test score schools. Moreover, the analysis also shows that increasing the revenues that schools receive for serving low-income students will not necessarily improve results for these students over those of other non-eligible students. This could either be because the program may not improve outcomes for low-income students; or because the benefits may be captured by all students who are similar in terms of wealth, regardless of whether they are eligible or not.

Finally, the paper also relates to the existing literature looking at behavioral barriers that prevent individuals from making what could be optimal educational choices. There is a broad literature that has looked at the barriers that students face when trying to apply to a postsecondary institution, including lack of information or the complexity associated with evaluating a substantial number of options ([Thaler and Mullainathan, 2008](#)). Both of which can drive students to make sub-optimal choices ([Radford, 2013](#); [Pallais, 2015a](#); [Smith et al., 2015a](#); [Thaler and Mullainathan, 2008](#); [Ross et al., 2013](#)). It has also been found that high-achieving low-income students tend to choose colleges that mimic the choices of

⁵see [Epple et al., 2015](#) for a more extensive review of this literature

their socioeconomically similar peers, despite being more academically advantaged ([Hoxby and Avery, 2013b](#)). Less attention has been given to the role that these factors play in school choice, given that there are few places with broad school choice systems. Still, some studies have shown that parents of each race prefer schools where their own race is the clear majority, implying that minority parents face much larger tradeoffs between academics and social preferences when choosing schools ([Hastings et al., 2009](#)).

III Institutional Background

A Voucher System

Chile has had a universal voucher system since 1981. In the system there are three types of schools: public schools that are managed by local municipalities and that represent approximately 38 percent of total enrollment, private voucher schools that represent approximately 54 percent of total enrollment, and unsubsidized private schools scattered to upper-income households that represent approximately 8 percent of total enrollment. Up until 2008 both public and private voucher schools were paid a flat voucher per student based on attendance.

The Chilean voucher scheme is quite unique in that it imposes few restrictions to private voucher schools. These schools can receive voucher subsidies regardless of their religious status, can operate for-profit, are allowed to implement admission policies subject to few restrictions, and as of 1994, can also charge add-ons to parents. These add-ons are capped at about three times the voucher payment, but this constraint is rarely binding.⁶ Instead, public schools face more restrictions; they are not allowed to charge add-ons to parents and cannot turn away students unless oversubscribed.

Based on the particularities of the Chilean voucher design, theoretical models such as [Epple and Romano \(1998\)](#) and [MacLeod and Urquiola \(2012, 2015\)](#) would suggest that the

⁶According to Urquiola and Verhoogen (2009) most of elite unsubsidized private schools could take vouchers but choose not to.

voucher system would lead to cream-skimming from the public sector, and stratification by income and/or ability within the private sector, which is exactly what is observed in practice.⁷

Although it is hard to isolate the causal effect of the voucher system on student sorting, existing research suggests that the voucher system has indeed led to increased socioeconomic stratification across schools. Among the relevant research, [Hsieh and Urquiola \(2003, 2006\)](#) implement a difference in difference strategy where they compare stratification measures across municipalities with more or less growth in the private sector, finding that the voucher-induced growth in the private sector was associated with a middle class exodus from public schools. In a related work, [McEwan et al. \(2008\)](#) compare students sorting across towns of different population size. Based on the idea that there is a minimum size required for private school entry and that towns close to this threshold are comparable, the authors find that private entry is related to higher student stratification.

There is a broader cross sectional literature in Chile that documents the high levels of stratification by socioeconomic status. [Valenzuela et al. \(2010\)](#) suggest that Chile has one of the highest levels of school-level stratification by socioeconomic status in the OECD. Moreover, [Mizala et al. \(2007\)](#) show that stratification is particularly extensive in the private sector.⁸

B Targeted Vouchers

The existing research on the high levels of segregation by socioeconomic status in the Chilean system was in part what led to the reforms that were implemented in 2008. The 2008 reform, *Ley de Subvención Escolar Preferencial*, introduced a targeted voucher for students belonging to the lowest 30 percent of the income distribution. Figure 1.1 shows how voucher amounts evolved in the period from 2006 to 2012 for eligible and non-eligible students. As can be

⁷In the theoretical model developed by [Epple and Romano \(1998\)](#) stratification occurs as a consequence of positive peer effects. Instead, in the model developed by [MacLeod and Urquiola \(2012, 2015\)](#) stratification occurs because employers use an individual's school of origin as a signal of her skill.

⁸see [Epple et al. \(2015\)](#) for a more extensive review of the Chilean voucher system

seen, in 2012, eligible students were receiving approximately 65 USD extra per month, which represented roughly a 50 percent increase over the regular voucher amount.

The extra resources were made available to all public schools, as well as private voucher schools that signed up for the policy. Schools that chose not to sign-up for the policy could still receive the regular voucher amount for each student, but could not receive the extra resources from targeted vouchers. In what follows I describe the type of schools that chose to join the policy. This analysis is key to a better understanding of how the policy changed the choice sets for eligible parents. I then proceed to provide some information on how schools might have spent these extra resources. This second analysis is relevant to understand how the policy might or might not have impacted educational outcomes.

Schools that signed up for targeted vouchers

Schools that signed up to receive targeted vouchers had to agree to: provide detailed accounting of the use of targeted voucher funds, something that is not required for the regular voucher; present an educational improvement plan to the Ministry of Education, with detailed education reforms that the school would undertake to improve test scores; define anticipated test score gains for future years, particularly for eligible students; eliminate screening of eligible students based on past academic performance and family background, although this was not enforced in practice; and not charge add-ons to eligible parents, although they could still charge add-ons to non-eligible parents.⁹

The policy, therefore, offered increased revenues for schools, but came at a cost. In practice by 2012, all public schools had chosen to join the policy, and approximately 95 percent of private voucher schools that charged no add-ons to parents had chosen to join. However, among private voucher schools that charged add-ons to parents only about 50 percent had decided to join. Figure 1.2 shows the amount of private voucher schools that in

⁹Carrasco et al. (2014) provide qualitative evidence on private voucher schools' admission policies. According to the authors common admission practices include performing "game sessions" for prospective students aimed at measuring skills that can predict good behavior, development and adaptation, as well as interviews to prospective parents.

2012 were charging monthly copayments between 2 USD and 200 USD. Blue bars indicate the number of schools in each category that had chosen to join the policy by 2012 and grey bars show the number of schools in each category that had chosen not to join the policy. As can be seen, the great majority of schools that were charging between 2 USD and 50 USD had chosen to join the policy, but few schools with monthly add-ons above 50 USD had decided to join.

Table 1.1 presents average characteristics for public schools, private voucher schools that charge no add-ons to parents, and private voucher schools that charge add-ons to parents and had chosen either to join or not to join the policy. All measures are for 2011, the year when parents in my sample made their choices.¹⁰ As can be seen, private voucher schools that charged high add-ons and/or served high SES students abstained from participating in the policy. Still, it can be seen from Table 1.1 that the policy gave low-income students free access to a subgroup of schools that in 2011 had higher test scores (their average academic performance was in the 66th percentile) and had higher SES students (the average SES status of their students was in the 78th percentile) than public schools and private voucher schools that charged no add-ons to parents.

Use of resources from targeted vouchers

To join the program, schools had to agree, among other things, to present an Educational Improvement Plan detailing the educational reforms that the school would undertake to improve academic results. The plan was also meant to set academic goals, particularly for eligible students. Educational Improvement Plans contained specific actions that schools would perform in areas such as: curriculum management, school leadership, student life, and resource management. Resources from targeted vouchers could be spent on, among other

¹⁰Because the policy was implemented in 2008 these measures may already reflect some of the changes induced by the policy. Appendix 1.B presents the average characteristics for these schools in 2007, the year before the policy was implemented. Data shows that in the period from 2007 to 2011, public schools and private voucher schools that had joined the policy increased their test scores. Still, it is always the case that private voucher schools with add ons that joined the policy are better than public schools and private voucher schools that charge no add-ons to parents.

things, hiring teachers, educational assistants, and the necessary staff to meet the goals of the Educational Improvement Plans. However, schools were not allowed to use these resources for increased salaries, bonuses and other expenditure categories such as debt repayment or school celebrations (Feigenberg et al., 2017).

The Ministry of Education classified schools in three categories according to their academic performance and the socioeconomic status of their students. Schools in higher categories had more freedom to define their Educational Improvement Plans and could decide how to allocate the resources from targeted vouchers. Instead, schools in the lowest category had to elaborate their Educational Improvement Plans with the assistance of the Ministry of Education. The sanction that schools could receive as a result of noncompliance with their Educational Improvement Plans depended on their classification. Schools in higher categories could be demoted to a lower category, and schools in the lowest category could eventually be closed. In practice, in 2008 when the policy was first implemented, no school was given the lowest category. By 2012, roughly 3 percent of schools had fallen to the lowest category, but none of them had been closed.

Even though the reform was meant to hold schools accountable for the extra resources from targeted vouchers, in practice a number of schools did not comply with the requirements, a point emphasized by Feigenberg et al. (2017). Information comes from an audit conducted by the Chilean Comptroller’s Office in 2012 that compared the funding inflows from targeted vouchers for the 2008 to 2011 period to documented expenditures in 77 of 345 municipalities. According to Feigenberg et al. (2017), on average only 65 percent of received funds could be linked to validated expenditures during the audit report.

IV Data and Descriptive Statistics

In this study I use a unique national dataset containing detailed information at the individual level. What makes the data unique is information on students’ socioeconomic ranking

(*Ficha de Protección Social*) for 2012, which is the variable used to determine program eligibility. This variable was provided by the Ministry of Education and the Ministry of Social Development and was not available for previous studies.

Information on program eligibility is merged with administrative records on school enrollment that cover the whole population of students. To characterize schools, I use administrative data on schools' locations, prices, type of holder (i.e., private vs. public), size, and class size. Data from the SIMCE, a nationwide test for 2nd, 4th, 6th, and 8th graders, which is taken once a year, is used to measure schools' academic achievement as well as students' educational outcomes.

To characterize students, I use information from questionnaires given to parents in 2nd, 4th, 6th, and 8th grade together with the SIMCE. These questionnaires allow me to gather information about: mother's education, father's education, number of books in the house, monthly income, and childcare attendance. This information is complemented with detailed enrollment records to document students' age, gender and exact location. Data on students' exact addresses comes from enrollment records, where schools report parents' addresses. This information is not available for the entire sample, but I am able to get exact addresses for approximately 40 percent of students.

For simplicity I restrict my analysis to students entering 1st grade in 2012. The choice to focus on 1st grade students has to do with analyzing students who are relatively free to choose any school and who face no costs associated with changing schools. Still, results remain the same when looking at students in grades 1 to 8.¹¹ Because the socioeconomic ranking changes from year to year, and I only have data on this variable for 2012, I need to restrict my analysis to 2012, four years after the policy was first implemented. The benefit of focusing on such a period is that by then we expect that parents and students must have had a good sense of how the policy actually worked.

¹¹Results available upon request

V Empirical Strategy

In order to estimate the causal effect of the differentiated voucher I exploit the fact that the targeted vouchers' eligibility process generates large discontinuities in the relation between the socioeconomic status ranking and the probability of being eligible. Although there are other criteria that can determine eligibility, the socioeconomic ranking accounts for about 83 percent of all participants in the program (i.e., it is the binding threshold).¹² This allows me to implement a fuzzy regression discontinuity design. The estimating equations can be written as:

$$y_i = \alpha_0 + \rho D_i + f(r_i) + \delta'X + \epsilon_i \quad (1.1)$$

$$D_i = \gamma_0 + \pi 1[r_i \leq r_0] + g(r_i) + \nu'X + \mu_i \quad (1.2)$$

where y_i is the variable of interest, for example, academic performance of student i , D_i equals one if the student is eligible for targeted vouchers, r_i is the running variable (students socioeconomic ranking), $1[r_i \leq r_0]$ is an indicator function that equals one if the student is below the threshold for program eligibility, X are students' socioeconomic characteristics, and ϵ_i and μ_i are error terms. Throughout the paper I use optimal bandwidths and robust confidence intervals proposed by [Calonico et al. \(2014\)](#). Because optimal bandwidths are estimated separately for each outcomes, the number of observations may vary depending on the outcome that is being studied.

The socioeconomic ranking used to determine eligibility is an instrument designed to measure the risk of being in poverty. It takes into account a household's ability to generate income based on education, experience, and county of residence, and a household's economic need based on, among other things, the number of children. The socioeconomic ranking is used to assign a number of other welfare programs and was created prior to the introduction

¹²Students can also qualify as beneficiaries if: (i) they belong to the social program *Chile Solidario* which is the component of the Social Protection System in charge of serving families, people, and areas in social vulnerability condition, or (ii) they meet certain poverty requirements. However, under (ii) students will only be beneficiaries for a year and will have to be reevaluated under the government's socioeconomic ranking during the next year in order not to lose the benefit

of targeted vouchers. This ranking can change from year to year, either because the ages of household members change or because households choose to be reevaluated to apply to other welfare programs, which typically require up-to-date information on the socioeconomic ranking. If a student falls above the cutoff on a given year, he or she will no longer be eligible for targeted vouchers. Importantly, the threshold for targeted vouchers does not overlap with the threshold for any other program.

VI Results

A First-Stage Estimates

Figure 1.3 plots targeted voucher assignment in 2012 as a function of the socioeconomic ranking in that same year. Black lines depict a fourth order polynomial fit for control and treatment units separately, and grey dots represent the sample average for each disjoint bin. Here, and in what follows, when talking about eligible students I will be referring to those students who were classified as eligible for targeted voucher by the Ministry of Education. In practice, not all of these students received the extra voucher amount because some of them chose to enroll in private schools or private voucher schools that didn't join the policy.

From Figure 1.3 it can be seen that students below the cutoff point are always eligible. However, the discontinuity is not sharp because students above the cutoff can still receive targeted vouchers if: (i) they belong to the social program *Chile Solidario* which is a component of the Social Protection System in charge of serving families, people, and areas in social vulnerability condition, or (ii) they meet certain poverty requirements. In theory, all students who belong to the program *Chile Solidario* will automatically be eligible, however parents who meet certain poverty requirements need to actively apply in order to become eligible.

In my sample, around 63% of students in the control group are not eligible, 15% of students in the control group are eligible because they belong to the program *Chile Solidario*,

and 22% of students in the control group are eligible because they meet certain poverty requirements. In theory students in this latter group should only be eligible for a year and should be reevaluated according to the socioeconomic ranking during the next year in order not to lose the benefit. However, in practice, we observe that only 32% of students above the cutoff who met the poverty requirement lost their benefit in 2013, 18% were reevaluated and met the socioeconomic ranking threshold, and 50% continued to be eligible for meeting the poverty requirement.

Table 1.2 presents estimates of the change in the probability of being eligible for targeted vouchers for students who are below the cutoff, where $R \leq Cutoff$ is a dummy variable that equals one if the individuals' socioeconomic ranking is below the cutoff for program eligibility. As can be seen, being below the cutoff increases the probability of receiving a targeted voucher by 70 percentage points.

B Balancing Checks

Next, I perform standard balancing checks to examine whether individuals just above and just below the cutoff are similar in terms of their observable characteristics. I look at a set of socioeconomic variables that should not be affected by the program. If the procedure is valid then RD estimates should be equal to zero.

Results are presented in Figure 1.4 and Table 1.3. Mother's and father's education equal total years of education; income equals monthly income in US dollars; books equal total number of books in the house; internet and computer are both dummies that equal one if the student had internet or computer in his or her house; and attended childcare is a dummy variable that equals one if the student attended childcare from 0 to 2 years old.

Figure 1.4 displays binned mean of observable characteristics of students by socioeconomic score relative to the cutoff. As can be seen, all of the studied characteristics change smoothly across the threshold. Regression estimates in Table 1.3 confirm the visual analysis; results are small in magnitude and precisely estimated, indicating that students around the

cutoff are similar in terms of their socioeconomic characteristics. I do observe that students below the cutoff are 2 percentage points more likely to attend childcare from 0 to 2 years old. However, the difference is small in magnitude and only significant at the 10 percent level.¹³

To further check whether there is any sign of socioeconomic score manipulation I proceed to look at whether there is any evidence of a visible jump in the density around the discontinuity. Figure 1.5 (a) shows a histogram of scores relative to admission cutoff value, and Figure 1.5 (b) shows the result from the McCrary (2008) test. As can be seen, there is no sign of a visible jump in the density around the discontinuity. The McCrary (2008) test confirms this, showing no evidence of a statistically significant break.¹⁴

The fact that I do not observe any evidence of score manipulation near the cutoff is to be expected. Eligibility for targeted vouchers is determined by the Ministry of Education based on administrative data, and it does not require an application on behalf of parents. Also, information on cutoff scores is not made available to parents. Although there is some anecdotal evidence of socioeconomic ranking manipulation, this tends to occur at other cutoff points, where other welfare programs, such as housing, are assigned.

C Targeted Vouchers and School Choice

I now turn to estimate the impact of being eligible for targeted vouchers on school choice. I expect that being eligible for a targeted voucher will: potentially expand the school choice sets for parents, and change the relative prices that parents have to pay for a school. Being eligible should expand choices because schools that join the program should now be more willing to accept eligible students as opposed to non-eligible students. It should also change relative prices because eligible students now do not have to pay add-ons to attend schools that joined the program.

¹³Socioeconomic variables are available for approximately 70% of students in my sample. Missing rates are also smooth around the cutoff

¹⁴The log difference in height being 0.014 with standard error of 0.020

The impact that targeted vouchers might have on enrollment decisions will ultimately depend on parents' preferences. In general, previous studies have found that when it comes to school choice, parents have a high valuation for proximity, and that preferences for test scores increase with students' income and own academic ability (Bayer et al., 2007; Hastings and Weinstein, 2008). Similar results have been found for Chile, where authors have documented that quality is a superior attribute and closeness to home an inferior attribute, suggesting that poor families tend to value more closeness to home than school quality (Gallego and Hernando, 2008; Chumacero et al., 2011).

In general, I expect that being eligible for targeted vouchers should lead parents to choose schools of higher test scores or higher socioeconomic status. However, because not all schools chose to join the policy, it is also possible that the change in relative prices leads parents to choose schools of lower test scores or lower socioeconomic status. This would be the case if, had they not been eligible, parents would have chosen a private school or a private voucher school that didn't join the policy. As a reference, in 2007, the year before the policy was implemented: approximately 57 percent of students entering 1st grade who met the requirement to be eligible for targeted vouchers enrolled in a public school; 16 percent enrolled in a private voucher school that charged no add-ons to parents; 18 percent enrolled in a private voucher school that charged add-ons to parents and that later joined the policy; and only 9 percent enrolled in a private or private voucher school that did not join the policy.¹⁵ Therefore, I may not expect to see much of a negative effect in terms of having parents choose schools of lower test scores or lower socioeconomic status.

Results are presented in Figure 1.6 and Table 1.4. Chosen schools are characterized based on their observable characteristics in 2011, which is the year before parents made their choices. Private is a dummy variable that equals one if the chosen school is private; test score equals the performance of the chosen school on 4th grade standardized test scores; socioeconomic status equals the average years of education of the mothers' of students at-

¹⁵These numbers are similar for students closer to the threshold

tending the chosen school; add-on equals the monthly amount charged by the chosen school to non-eligible parents in USD; class size equals the average class size of the chosen school; and distance equals the distance traveled to school in miles.¹⁶ Schools' test scores and socioeconomic status are both standardized by the mean and standard deviation of the variables in 2005.

Figure 1.6 (a) to (f) display binned mean of observable characteristics of the schools chosen by parents by students' socioeconomic score relative to the cutoff. As can be seen, all of the studied school characteristics change smoothly across the threshold. Estimates in Table 1.4 Columns (1) to (6) confirm the visual analysis. Panel A contains reduced form estimates and Panel B contains instrumental variable estimates, where the discontinuity is used as an instrument for being eligible for a targeted voucher. All estimates include controls for mother's education, father's education, household income, and region. Results remain the same when using alternative bandwidths (see Table 1.C.1). As can be seen, being below the cutoff has no impact on: the probability of choosing a private school, the test scores of the chosen school, the socioeconomic status of the chosen school, the average class size of the chosen school, or the distance traveled to school. Results do show that students below the cutoff choose schools that charge higher add-ons to non-eligible parents. However, these results are small in magnitude, indicating that eligible parents choose on average schools that charged 3 USD more per month from an average monthly cost of 15 USD for students in the control group.

D Targeted Vouchers and Educational Outcomes

I next turn to estimate the impact of being eligible for targeted vouchers on students' educational outcomes. Although the program had no impact on parents' school choices, it is still possible that the program had a positive impact on educational outcomes. First of all, it is possible that the program allowed parents to choose schools that better meet their

¹⁶I am only able to get exact location for approximately 40 percent of students in my sample which is why there are fewer observations in distance estimates.

children's needs, albeit there are no significant differences in the observable characteristics of the chosen schools. Second, because schools received extra revenues as a result of the policy, they could have used these resources to improve the educational outcomes of eligible students.

Results for educational outcomes are presented in Figure 1.6 (g) to (i) and Table 1.4 Columns (7) to (9). Figure 1.6 (g) to (i) display binned mean of test scores by students' socioeconomic score relative to the cutoff, and Table 1.4 Columns (7) to (9) present estimates of students' performance in a language test that is applied nation-wide to students in 2nd and 4th grade, and a math test that is applied nation-wide to students in 4th grade. Test scores for 4th grade are standardized by the mean and standard deviation of these variables in 2005. Test scores for 2nd grade are standardized by the mean and standard deviation of the variable in that same year, because there is no measure of second grade test scores for previous years. As can be seen, results show no impact of the program on students' educational outcomes in the short and medium term. Coefficients are all negative and non-significant. Moreover, I can reject in all cases a positive impact on test scores above 0.04 standard deviations.

Because students in the control group may become eligible in subsequent years, I perform a second analysis where I use the socioeconomic ranking as an instrument for the number of years that the student has been eligible to look at educational outcomes in the short and mid-term. Results can be found on Table 1.5. As can be seen in Panel A, being above the threshold for targeted vouchers increases the number of years that students are eligible by approximately 0.7 years. In line with previous results coefficients are all negative and non-significant, with the exception of 4th grade math results that are negative and significant at the 10 percent level.

E Understanding the Null Effects

To better understand whether there are barriers that could impede low-income students from attending higher socioeconomic status or higher test score schools, I proceed to look at heterogeneous effects across various margins. I begin by looking at whether effects differ by mothers' educational level. Previous literature has shown that parents' preferences for school characteristics tend to vary with socioeconomic status ([Bayer et al., 2007](#); [Hastings and Weinstein, 2008](#)). Also, in the Chilean context where schools can implement admission policies subject to few restrictions, higher quality schools could choose to serve, among eligible students, those of higher socioeconomic status, leading to a differential impact of the program.¹⁷

Table 1.6 presents heterogeneous effects by mothers' education. In general results show no impact on the choices made by mothers with less than high school education, and mother's with high school education or tertiary education. No impact can be found on the test scores of the chosen school, the socioeconomic status of the chosen school, the average class size of the chosen school, or the distance traveled to school for any of these groups. Estimates suggest that the impact on the prices of the schools chosen by parents might be higher for students with more educated mothers. However, estimates are not precise enough to reject the hypothesis that the impact is the same for both groups of students. Results in columns (7) to (9) also show no impact on educational results for students with more or less educated mothers.

Next, I look at heterogeneous effects by distance to the nearest private voucher school that charges add-ons to parents and joined the policy. It is possible that the policy did not have an effect on students because there were distance barriers that were preventing them from switching to private voucher schools with higher test scores or higher socioeconomic status. To explore whether there is any evidence in favor of this hypothesis, I run a regression for the subgroup of students who have at least one private voucher school that charges add-

¹⁷Robustness checks to supplement the heterogeneity analysis can be found in Appendix 1.D.

on to parents and joined the policy in less than 0.4 miles. I choose to look at heterogeneous effects by distance to this specific group of schools, because I believe that most of the policy’s effect should come from giving low-income parents access to this group of private voucher schools that charge add-ons to non-eligible parents and that are now free for low-income students. As a point of reference, the distance travelled by 1st grade students in 2012 had a mean of 1.3 and a median of 0.73 miles. Because I do not have data on exact locations for all students in my sample, I have to restrict the analysis to the 40 percent of students for whom I do have information on exact addresses.¹⁸

Table 1.7 presents heterogeneous effects by distance to the nearest private voucher school that charges add-ons and joined the policy. As can be seen, there is no evidence of a differential impact of the program on the probability of choosing a private school, the test scores of the chosen school, the socioeconomic status of the chosen school, the average class of the chosen school, or the distance traveled to school for students who live closer to this sub-group of schools. I do observe that the program seems to have had a higher impact on the probability of choosing a school that charges higher add-ons to non-eligible parents, for students living closer to a private voucher school that charges add-ons and joined the policy, however, the difference between groups is non-statistically significant.

In line with previous results, I observe that the program did not have a differential impact on educational outcomes for students living close to a private voucher school that charges add-ons to parents and joined the policy. Results for educational outcomes can be found in Table 1.7 in columns (7) to (9).

Finally, I proceed to look at whether results look different further away from the discontinuity. Extending results beyond the discontinuity is useful for the analysis for two main reasons. First, although the regression discontinuity design provides a credible identification strategy there might be concern that, because the socioeconomic status score can vary from year to year, uncertainty with respect to next years’ eligibility status could be driving the null

¹⁸The lack of data on exact addresses for the whole sample of students also prevents me from doing exercises with alternative distances

results. Uncertainty could be especially relevant for individuals close to the cutoff who are in the margin of becoming eligible or ineligible. In practice, data indicates that 95 percent of students in the sample who were close to the cutoff and who were eligible in 2012 continued to be eligible in 2013. However, 65 percent of students close to the cutoff in the sample who were ineligible in 2012 became eligible in 2013. A major concern, therefore, is that ineligible students close to the cutoff might foresee that they are likely to become eligible in the future. Extending results beyond the discontinuity allows me to address this concern by including a broader control group that is unlikely to become eligible in the future. Only 17 percent of students who were ineligible in 2012 became eligible in the following year and only 2 percent of eligible students in 2012 lost their benefit in the following year.

Second, extending results beyond the discontinuity also allows me to determine whether results look any different for students who are further away from the cutoff. Students close to the cutoff are approximately in the 40th percentile in terms of socioeconomic status. Results could be different for students in the bottom of the distribution for a number of reasons: changes in relative prices could be more relevant for this group of students; they may face a different supply of schools in their neighborhoods; or they may have different preferences for school attributes. The same could be true for students who are further above the threshold.

To extend my results beyond the discontinuity I follow [Angrist and Rokkanen \(2015\)](#) and exploit the availability of dependent variable predictors other than the running variable, to estimate the causal impact of targeted vouchers for students who are away from the cutoff. The basic idea is that the link between the running variable and outcomes can be broken by conditioning on a relevant set of controls. The running variable r_i can be thought of as a function of two parts $g(x_i, \epsilon_i)$, where x_i is observed and ϵ_i is not. Conditional on x_i the only source of variation in r_i , and consequently in eligibility, is ϵ_i . Thus, the conditional independence assumption requires that, conditional on the observed x_i , potential outcomes are mean-independent of unobserved determinants of the running variable.

The strategy used is similar to a conventional matching strategy, where the conditional

independence assumption helps to break the link between treatment status and potential outcomes. However, the approach represents an improvement over the conventional matching strategy because it uses the information inherent in the regression discontinuity design to guide the choice of the conditional vector x_i and to test the veracity of the conditional independence assumption.

Following [Angrist and Rokkanen \(2015\)](#), I construct a conditional vector that includes controls for mothers' education, fathers' education, household income, municipality of residence, and whether the household had access to internet and computer. I choose a conditional vector that breaks the link between the running variable and outcomes while preserving the common support required for the matching strategy. In practice this can be done by choosing a vector such that, once I control for this vector, the running variable does not predict changes in the outcome variables at either side of the cutoff.

Using this set of control variables, I am able to break the link between the running variable and outcome variables for students who are in a window of -3000 to 0 points around the cutoff, and significantly reduce the link for students who are in a window of 0 to 8000 points around the cutoff. This represents approximately 54 percent of students in my sample. I choose not to include student beyond this window because there is evidence of socioeconomic score manipulation for students who are further away (see [Figure 1.5](#)). For households who are outside of my window, the running variable might be reflecting things aside from socioeconomic indicators, such as parents' ability to manipulate the socioeconomic score. This strategy helps to extend results for a larger sample of students. Regression discontinuity estimates typically include 13 percent to 20 percent of students in the sample.

Results for the conditional independence test can be found in [Table 1.8](#). Panel A and Panel C show the relationship between the running variable and the outcomes of interest for students who are below and above the cutoff. The running variable is divided by 1000, meaning that, for example, a 1000 increase in the socioeconomic score predicts a 0.014 standard deviation increase in the test scores of schools chosen by parents for students who

are below the threshold. Next, Panels B and D show this relationship once I control by the set of socioeconomic indicators. As can be seen in Panel B, including the set of controls significantly reduces the relationship between the running variable and the outcomes of interest for students who are below the cutoff. Aside from distance and class size, all the other coefficients are significantly reduced and are no longer significant. Estimates for students below the cutoff are used to determine what the outcomes would have looked like for students above the threshold had they been treated.¹⁹

Results are less encouraging for students who are above the cutoff. Although the relationship between the running variable and the outcomes of interest is significantly reduced once I include controls, the estimates are still statistically significant. Estimates for students above the threshold are used to predict what the outcomes for students below the threshold would have looked like had they not been treated. Thus, the estimates for students below the cutoff may be slightly downward biased. This shouldn't be much cause for concern given that the coefficients are small in magnitude.

I further complement this formal conditional independence assumption testing with a graphical tool that looks at the relationship between outcome residuals -after regressing outcomes on the conditional vector- and the running variable (Angrist and Rokkanen, 2015). If the conditional independence assumption is correct, then the relationship between outcome residuals and the running variable should be flat, except possibly for a jump at the cutoff. Results can be found in Figure 1.7, black lines depict a fourth order polynomial fit for control and treatment units separately, and grey dots represent the sample average for each disjoint bin. Consistent with the results reported in Table 1.8, the relationship between outcome residuals and running variable is essentially flat below the cutoff, except for distance. However, for individuals above the threshold, there is a small positive relationship between the running variable and residuals.

¹⁹Because the set of controls does not completely eliminate the relationship between class size, distance and the running variable, these estimates should be interpreted a little more carefully. Estimates for class size may be downward biased for students above the threshold, and estimates for distance may be upward biased for students above the threshold.

Results in Table 1.8 and Figure 1.7 indicate that the strategy will allow me to credibly determine what the outcomes would have been for students above the threshold, had they been treated. However, results on what the outcomes would have been for student below the threshold had they not been treated may be slightly upward biased, leading me to underestimate the impact of the treatment. This is because the conditioning vector is not able to fully remove the relationship between the running variable and the outcome of interest for individuals who are above the threshold.

Having evaluated the robustness of the conditional independence assumption, I proceed to estimate my results using linear reweighting and propensity score weighting. Results for the linear reweighting estimator are based on Kline (2011). Kline’s reweighting estimator begins with linear models for conditional means, which can be written:

$$\begin{aligned} E[y_i|x_i, D_i = 0] &= x_i' \beta_0 \\ E[y_i|x_i, D_i = 1] &= x_i' \beta_1 \end{aligned} \tag{1.3}$$

This leads to the following matching style estimator at specific running variable values:

$$E[Y_{1i} - Y_{0i}|r_i = c] = (\beta_1 - \beta_0)' E[x_i|r_i = c] \tag{1.4}$$

Table 1.9 reports linear reweighting estimates of average treatment effects. I estimate both the average treatment effect on the treated (Panel A) and the average treatment effect on the un-treated (Panel B). Consistent with previous results, coefficients are very small in magnitude indicating that being below the cutoff for targeted vouchers didn’t have an impact on this broader sample of students. In line with previous finding, results do show that targeted vouchers can lead parents to choose schools that charge higher add-ons to non-eligible parents. I also observe a negative effect on class size, and students’ performance on language second grade tests. However, although significant, these coefficients are very

small in magnitude indicating that access to targeted vouchers could have led parent to choose schools that are 1 percent smaller, and could have decreased test scores for students in 0.02 standard deviations. For students above the threshold, I observe that if they had been below the cutoff for targeted vouchers they would have chosen schools with somewhat higher add-ons, and done worse on second grade language tests by 0.03 standard deviations.

Figures 1.9 and 1.10 provide a visual evaluation of previous results by plotting linear reweighting estimates of $E[Y_{0i}|r_i = c]$ and $E[Y_{1i}|r_i = c]$ for all values of c . In Figure 1.9 the estimates of $E[Y_{1i}|r_i = c]$ to the left side of the cutoff (grey line) are fitted values from regression models for observed outcomes, while the estimates of $E[Y_{0i}|r_i = c]$ (blue line) are an extrapolation based on equation 1.3. Instead, in Figure 1.10 the estimates of $E[Y_{0i}|r_i = c]$ to the right side of the cutoff (grey line) are fitted values from regression models for observed outcomes, while the estimates of $E[Y_{1i}|r_i = c]$ (blue line) are an extrapolation.

The conditional means in the figures were constructed by plugging individual values of x_i into Equation 1.3 and smoothing the results using local linear regression. The figures present a picture consistent with that suggested by the estimates in Table 1.9, that is, small effects along all measured outcomes.

Finally, I complement previous results with a propensity score estimate approach. The logit model for the propensity score incorporates the control variables and parametrization used to construct tests in Table 1.8. The estimated propensity score distribution for individuals above and below the cutoff exhibits a substantial degree of overlap. This is documented in Figure 1.8, which plots the histogram of propensity score fitted values for treated and control observations above and below a common horizontal axis. The propensity-score-weighted estimates reported in the bottom half of Table 1.9 (Panels A and B), are consistent with the linear reweighting estimates shown in the first row of the Table.

VII Conclusion

Much of the debate over school vouchers revolves around the idea that voucher systems may lead to high levels of socioeconomic stratification. This is undesirable from a public perspective because socioeconomic segregation typically conveys school inequities and a loss in social cohesion. School inequities arise because schools with higher socioeconomic status students benefit from positive peer effects, higher quality teachers, more involved parents and more economic resources. At the same time, a loss in social cohesion occurs because segregation prevents students from different socioeconomic status from sharing a common experience in schools.

A question that remains open in the literature is whether, and to what extent, alternative voucher designs can help to overcome the socioeconomic segregation that is typically associated with voucher programs. In this paper I am able to address this question by looking at a reform in Chile where voucher amounts were increased by 50% for students in the lowest 40% of the income distribution. Using a unique dataset, I am able to exploit the fact that eligibility for targeted vouchers in Chile is a discontinuous function of a socioeconomic ranking and implement a regression discontinuity design. This allows me to estimate the impact that being eligible for a targeted voucher had on parents' school choices, and their consequential distribution across schools, as well as on eligible students' educational results.

Results show that being eligible for a targeted voucher had no impact on the observed characteristics of the schools chosen by parents. It had no impact on parents' probability of choosing a private school, the test scores of the chosen school, the socioeconomic status of the chosen school, the size of the chosen school, the average class size of the chosen school, or the distance traveled to school. Although I do observe that eligible parents choose schools that charge higher add-ons to non-eligible parents, the magnitude of this effect is negligible. There is also no evidence that eligible students are doing better than non-eligible students on a language test that is applied to second and fourth graders or a math test that is applied to fourth graders.

Two important conclusion can be drawn from this paper. First, eligible parents did not respond to the policy by choosing schools with significantly better observable characteristics. A result I argue is driven by both demand and supply side mechanisms. On the supply side, I observe that high test score private voucher schools abstained from participating in this policy. On the demand side I argue that low-income parents face other barriers, aside from costs and distance, that prevent them from attending higher socioeconomic status or higher test score schools. Barriers could include lack of information, complexities associated with evaluating a substantial number of school options, or issues of social belonging that lead parents to choose schools where their own social class is majority. Second, educational results did not improve in the eligibility margin, suggesting that schools did not respond to the policy by devoting more resources to eligible as opposed to non-eligible students.

Previous results contribute greatly to the empirical discussion on the role that targeted vouchers played in improving the educational outcomes of low-income students in Chile (MINEDUC, 2012; Correa et al., 2014; Villarroel, 2012; Mizala and Torche, 2013; Neilson, 2013; Navarro-Palau, 2017; Feigenberg et al., 2017). Using an improved identification strategy this paper is able to show that differentiated vouchers did not change students' distribution across schools and that they did not lead to an improvement in educational outcomes for eligible as opposed to non-eligible students. Further research is needed to determine whether the extra resources from this program did or did not contribute to the general increase in test scores for low-income students experienced during this period. However, this paper provides new evidence on the mechanisms that could be at work.

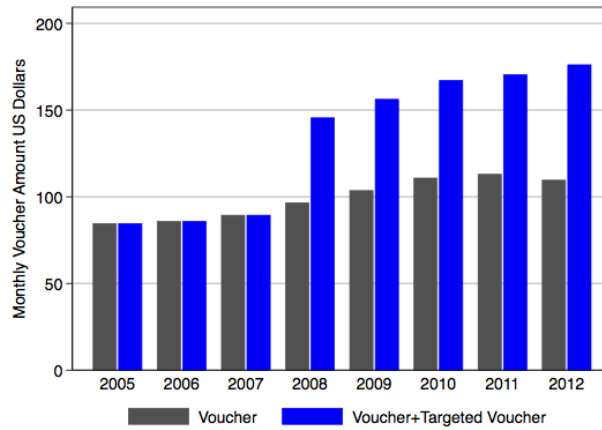
Findings are also extremely relevant from a policy perspective. The Chilean 2008 reform is one of the most important educational reforms to have been implemented in Chile in the last years, and efforts continue to be made to reduce the potential barriers that could be keeping low-income students from attending better performing private voucher schools. Previous results speak about the need for further intervention in order for progressive vouchers to help reduce socioeconomic stratification. In particular, it stands out the importance of enhancing

private voucher school participation, either through mandatory regulation or others. It also stands out the need to implement information campaigns that can reduce some of the barriers that parents face when choosing a school, as it has been shown that even the more educated parents who have good private voucher schools within a reasonable distance, did not respond to the price decrease by choosing better schools. Other details that would be worth considering have to do with the the level of understanding that parents have about the program and how parents weigh the risk of losing the benefit. As it is possible that parents in Chile might have a poor understanding of how the policy actually works, or that they might be afraid of losing their benefit from one year to the next.

The Chilean experience is also highly relevant for a number of other countries considering an expansion of school choice systems. Empirical studies in Chile have shown how voucher systems can lead to increased segregation by socioeconomic status ([Hsieh and Urquiola, 2003, 2006](#); [McEwan et al., 2008](#)). This study takes a step forward and is able to show that, without further intervention, alternative voucher designs, such as progressive vouchers, may prove to be ineffective in terms of addressing the high levels of socioeconomic segregation that may come as a result of voucher systems.

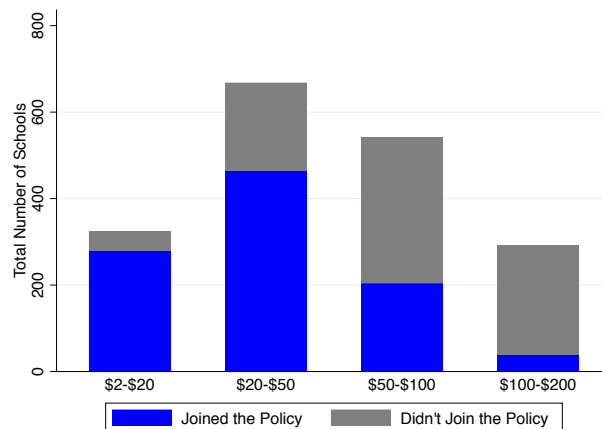
VIII Figures

Figure 1.1: Voucher Amounts over Time



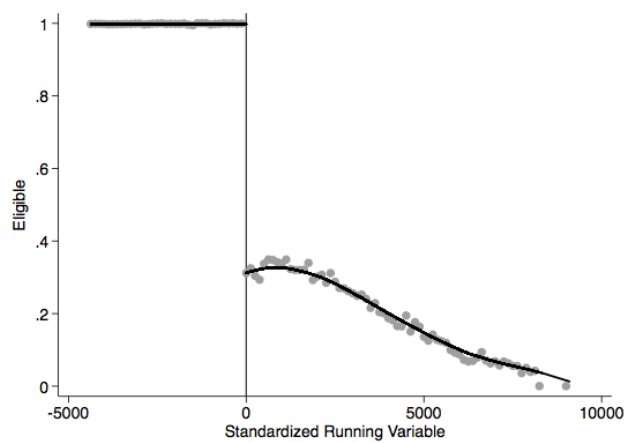
This figure shows how the voucher amount evolved over time for students who were non -eligible for targeted vouchers and students who were eligible for targeted vouchers. All amounts are in 2012 US dollars and represent a month of transfer. The voucher presented is for students in first grade at schools with full shift. Source: Ministry of Education.

Figure 1.2: Private Voucher Schools that Had and Had Not Joined the Policy in 2012 by Monthly Add-ons Charged to Parents



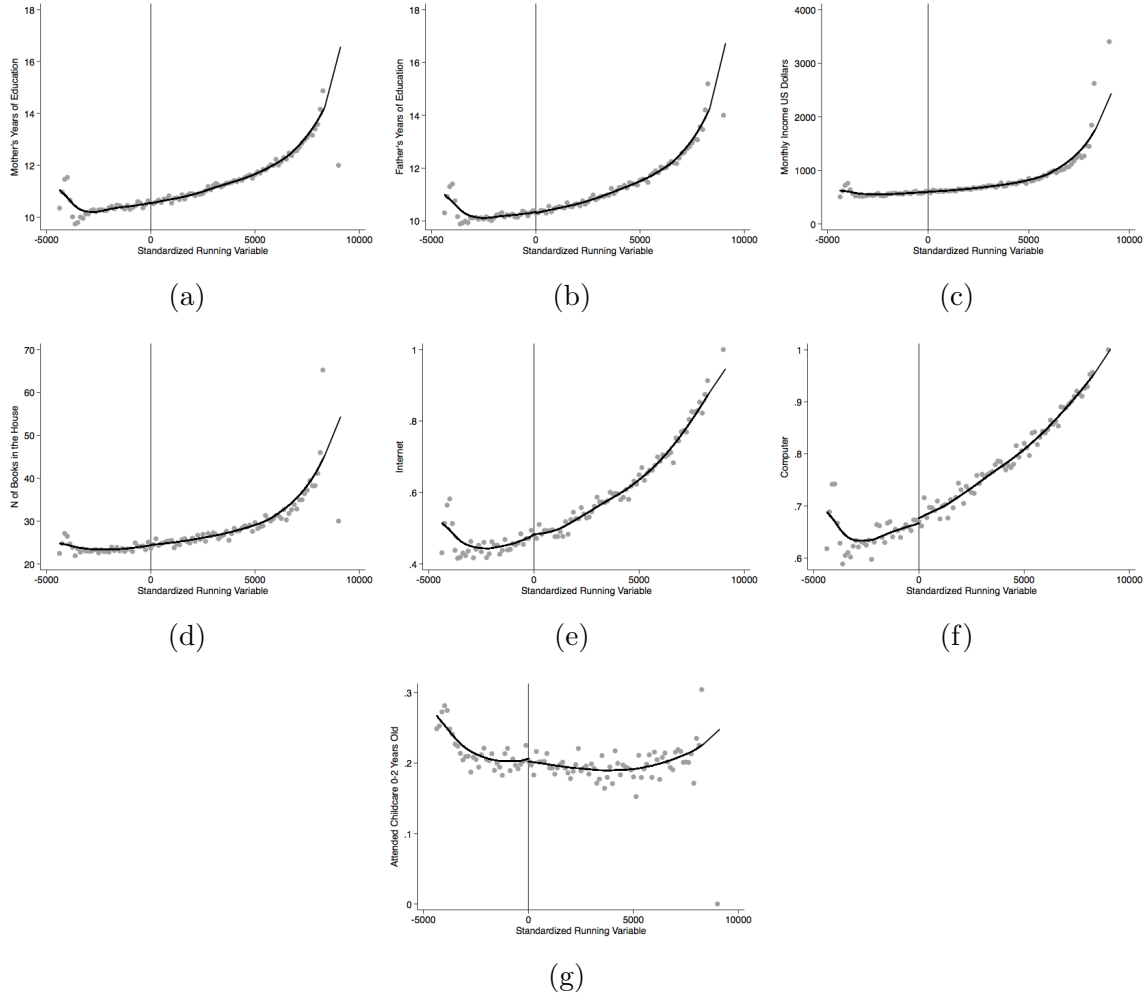
This figure shows the number of private voucher schools serving primary education that had and had not joined the policy in 2012 depending on the monthly voucher that they charged to parents. Source: Ministry of Education.

Figure 1.3: First Stage



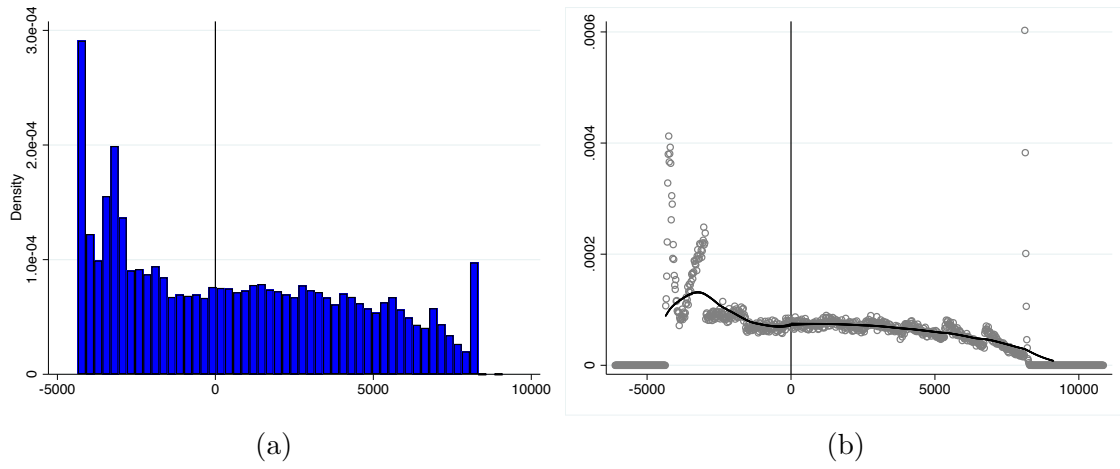
Grey dots present the average in the outcome variable for individuals in equally spaced disjoint bins. Black lines depict a fourth order polynomial fit for control and treatment units separately.

Figure 1.4: Visual Evaluation of Robustness Checks I



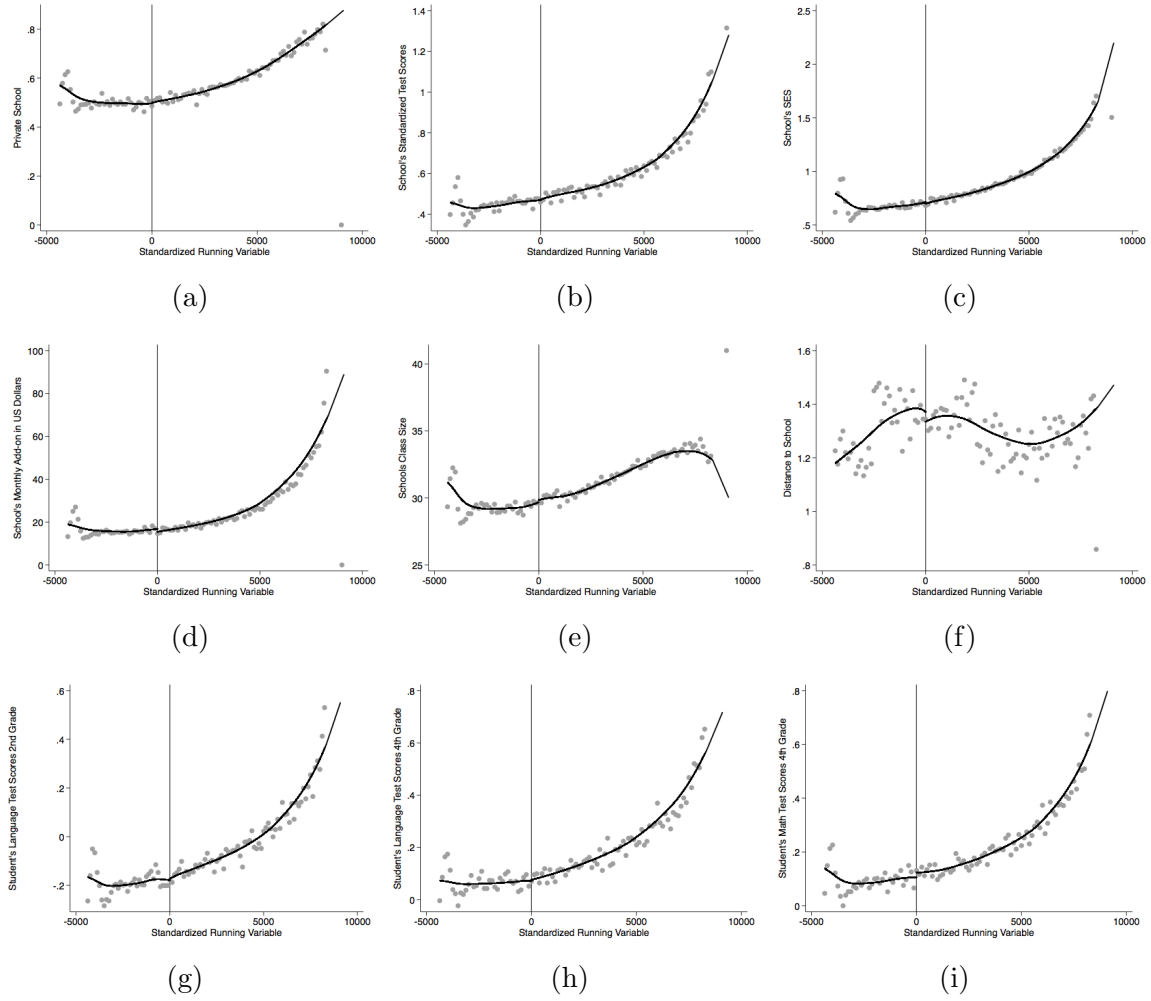
Grey dots present the average in the outcome variable for individuals in equally spaced disjoint bins. Black lines depict a fourth order polynomial fit for control and treatment units separately.

Figure 1.5: Visual Evaluation of Robustness Checks II



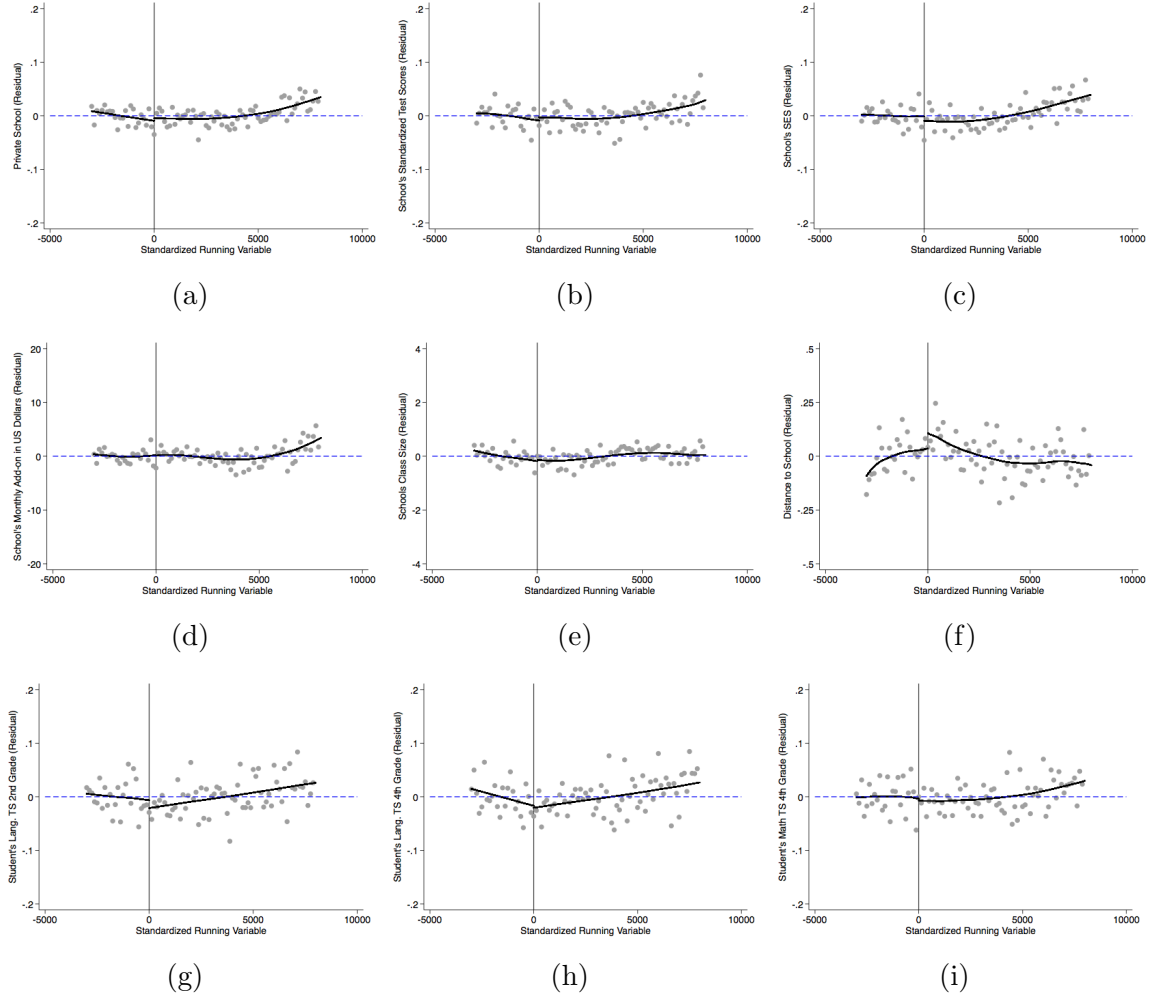
Notes: McCrary $r(\text{bandwidth}) = 1742.61$ $r(\text{binsize}) = 17.373$ $r(\text{se}) = .020256$ $r(\text{theta}) = .01413$

Figure 1.6: Visual Evaluation of Results



Grey dots present the average in the outcome variable for individuals in equally spaced disjoint bins. Black lines depict a fourth order polynomial fit for control and treatment units separately.

Figure 1.7: CIA Test



Grey dots present the average in the outcome residuals (after regressing outcomes on the conditional vector) for individuals in equally spaced disjoint bins. Black lines depict a fourth order polynomial fit for control and treatment units separately.

Figure 1.8: Histogram of Estimated Propensity Score in the Window $[-3000, 8000]$

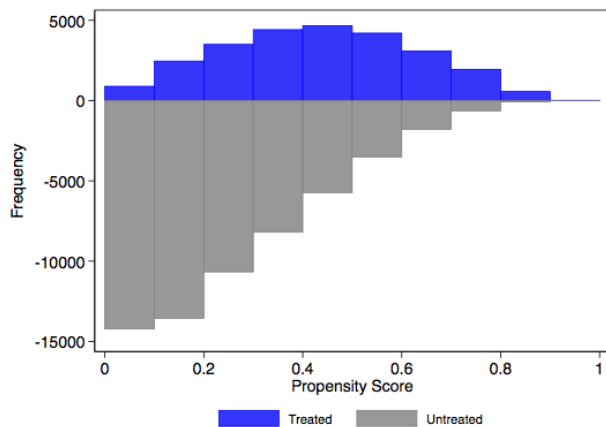
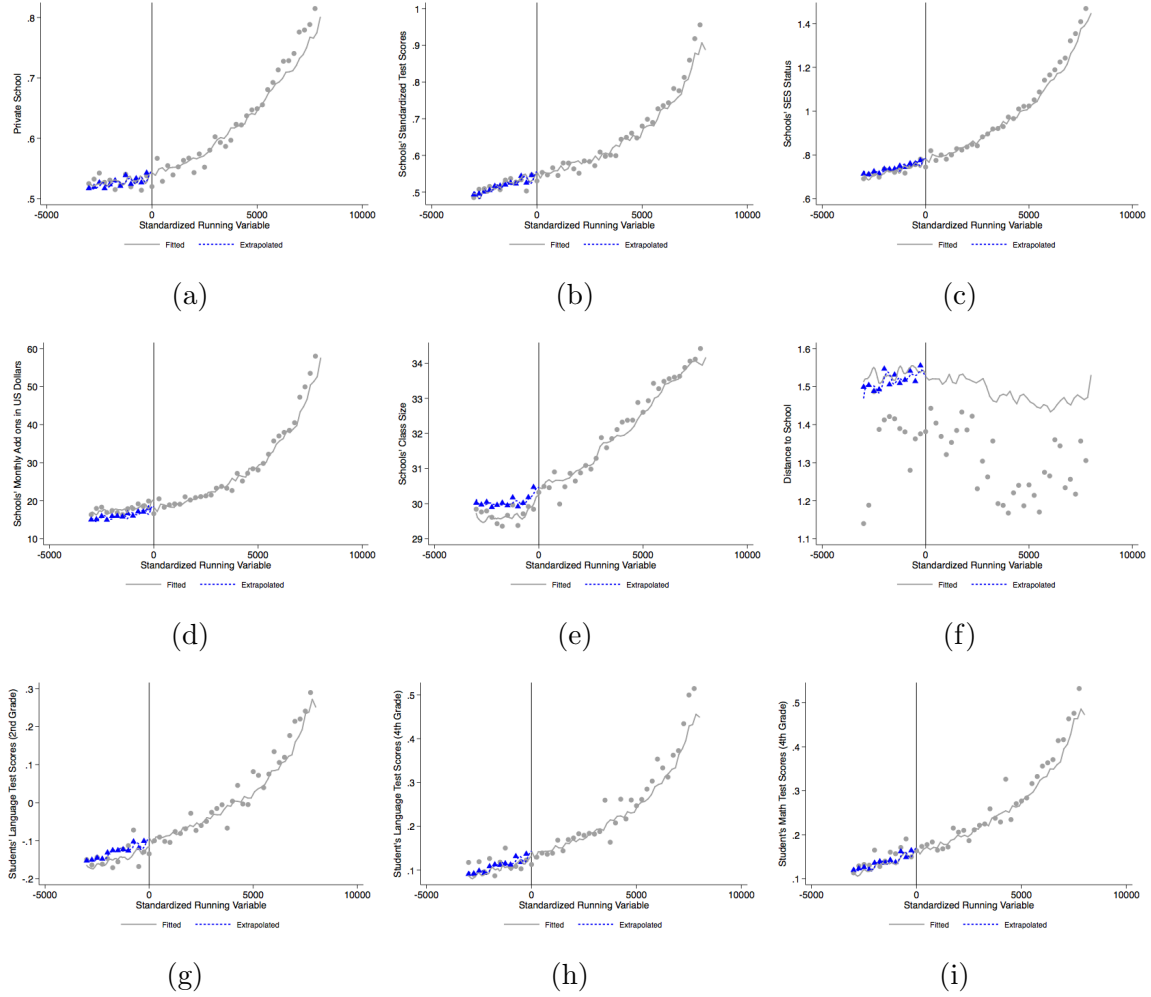
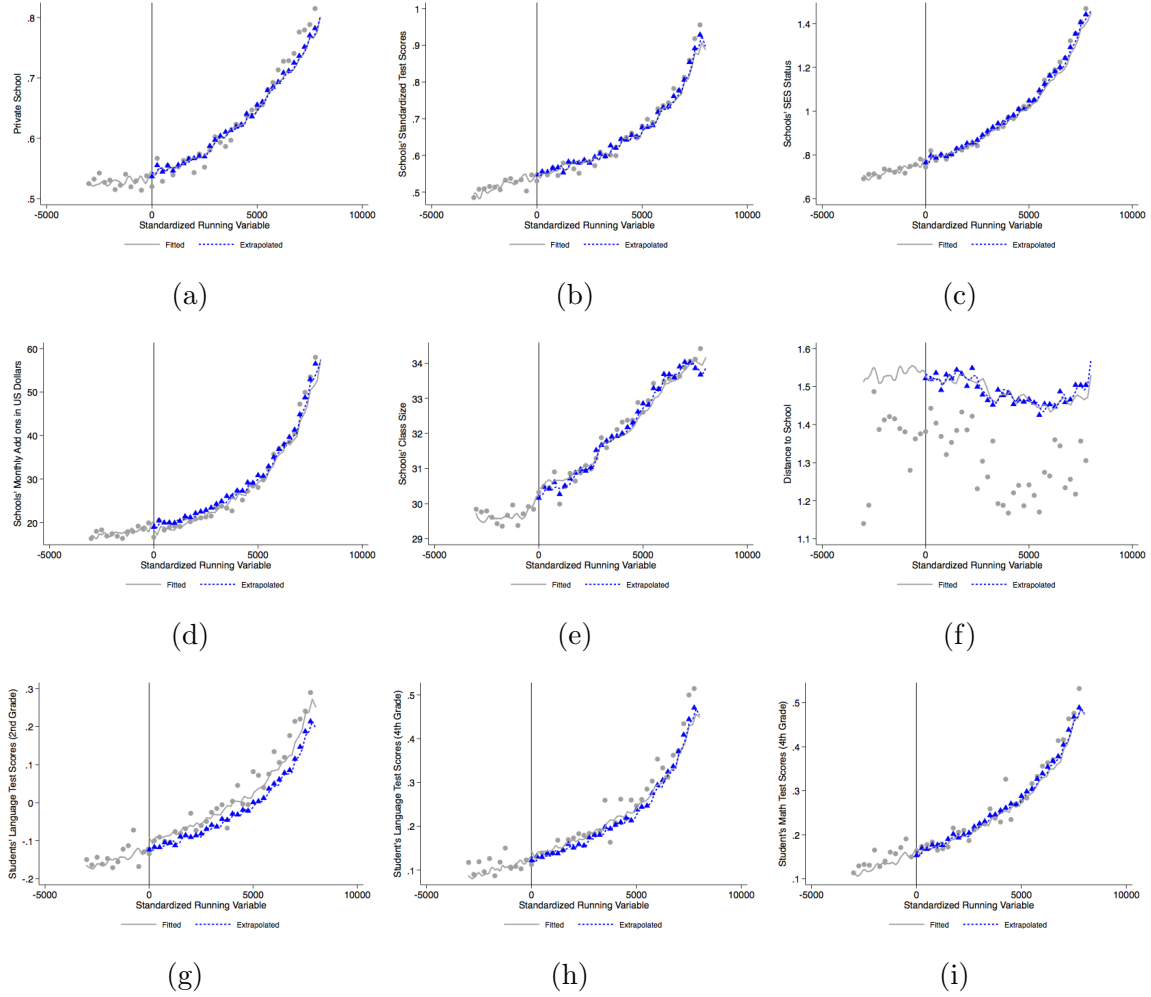


Figure 1.9: CIA-based Estimates Below the Cutoff



Conditional Independent Assumption based estimates of $E[Y_{0i}|r_i = c]$ and $E[Y_{1i}|r_i = c]$ for all values of c . Estimates of $E[Y_{1i}|r_i = c]$ to the left side of the cutoff (grey line) are fitted values from regression models for observed outcomes, while the estimates of $E[Y_{0i}|r_i = c]$ (blue line) are an extrapolation.

Figure 1.10: CIA-based Estimates Above the Cutoff



Conditional Independent Assumption based estimates of $E[Y_{0i}|r_i = c]$ and $E[Y_{1i}|r_i = c]$ for all values of c . Estimates of $E[Y_{0i}|r_i = c]$ to the right side of the cutoff (grey line) are fitted values from regression models for observed outcomes, while the estimates of $E[Y_{1i}|r_i = c]$ (blue line) are an extrapolation.

IX Tables

Table 1.1: Schools' Characteristics

	(1) Test Scores	(2) Test Scores Language	(3) Test Scores Math	(4) SES	(5) Add-on	(6) Size	(7) Class Size
Public (57%)	249.8 (27.7)	256.2 (28.3)	242.8 (31.1)	9.0 (1.8)	0.0 (0.0)	23.3 (25.7)	16.5 (12.6)
Private Voucher w/No Add-On that joined the policy (19%)	247.4 (28.1)	256.0 (27.6)	238.4 (32.5)	9.1 (2.2)	0.0 (0.0)	25.1 (28.3)	18.0 (14.2)
Private Voucher w/Add-On that joined the policy (13%)	261.6 (21.3)	266.0 (20.4)	256.8 (23.7)	11.6 (1.1)	41.0 (31.3)	53.4 (38.5)	31.6 (9.9)
Private Voucher w/Add-On that didn't join the policy (11%)	272.9 (19.6)	276.8 (18.6)	268.8 (22.0)	12.9 (1.1)	86.0 (47.0)	55.7 (39.5)	30.5 (9.5)

Includes all subsidized primary schools in 2012. Test score equals the average result of the schools on the 4th grade standardized test in 2011, SES equals the average years of education of mothers' of students attending those schools, add-on equals the total amount charged to non-eligible parents in those schools, school size equals the cohort size at those schools, and class size equals the average class size at those school.

Table 1.2: First Stage 2012

	Eligible
$R \leq Cutoff$	0.695 (0.00843)
Mean Control	0.304
Observations	28,119

Results from `rdrobust` (Calonico et al., 2014).

Table 1.3: Robustness Check

	(1) Mother's Education	(2) Father's Education	(3) Income	(4) Books	(5) Internet	(6) Computer	(7) Attended Childcare (0-2)
$R \leq Cutoff$	0.0473 (0.0956)	0.0644 (0.0964)	-10.79 (15.23)	0.266 (0.663)	0.00378 (0.0157)	0.00343 (0.0158)	0.0235 (0.0130)
Mean Control	10.51	10.31	605.1	24.56	0.484	0.672	0.198
Observations	17,360	18,167	21,572	24,796	19,066	16,612	18,366

Results from `rdrobust` (Calonico et al., 2014). Standard errors in parenthesis.

Table 1.4: School Choice and Educational Outcomes

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Panel A: Impact of Being Below the Cutoff (Reduced Form)									
$R \leq Cutoff$	0.00655 (0.0128)	0.00919 (0.0180)	0.0241 (0.0169)	2.060 (0.758)	0.0599 (0.283)	0.0379 (0.0702)	-0.0242 (0.0236)	-0.0163 (0.0239)	-0.0308 (0.0219)
Mean Control	0.497	0.467	0.693	15.31	29.72	1.356	-0.170	0.0827	0.135
Observations	26,335	27,695	23,519	30,426	23,339	10,927	32,625	26,748	27,064
Panel B: Impact of Being Eligible for a Targeted Voucher (IV)									
Eligible	0.00629 (0.0173)	0.00553 (0.0238)	0.0339 (0.0263)	2.978 (1.104)	0.0542 (0.369)	0.0502 (0.0995)	-0.0373 (0.0349)	-0.0252 (0.0323)	-0.0393 (0.0292)
Mean Control	0.497	0.467	0.693	15.31	29.72	1.356	-0.170	0.0827	0.135
Observations	30029	32964	20515	29794	28532	10264	30691	29870	31048

Results from `rdrobust` (Calonico et al., 2014). All estimates include controls for mother's education, father's education and region. Panel A contains reduced form estimates and Panel B contains instrumental variable estimates, where the discontinuity is used as an instrument for being eligible for a targeted voucher.

Table 1.5: Educational Outcomes

	(1) Student Language 2nd Grade	(2) Student Language 4th Grade	(3) Student Math 4th Grade
Panel A: First Stage-Years Eligible			
$R \leq Cutoff$	0.643 (0.0147)	0.748 (0.0346)	0.748 (0.0346)
Mean Control	1.329	2.932	2.932
Observations	15,507	10,866	10,866
Panel B: Impact of Being Eligible for an extra year of Targeted Voucher (IV)			
Years Eligible	-0.0432 (0.0412)	-0.00955 (0.0362)	-0.0634 (0.0380)
Mean Control	-0.170	0.0827	0.135
Observations	24948	18512	15076

Results from `rdrobust` (Calonico et al., 2014). All estimates include controls for mother's education, father's education and region. Panel A contains first stage estimates and Panel B contains instrumental variable estimates, where the discontinuity is used as an instrument for number of years eligible for a targeted voucher.

Table 1.6: School Choice and Educational Outcomes: Heterogeneous Effects by Mothers' Education

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Mother has less than High School Education									
Eligible	-0.0439 (0.0297)	0.0586 (0.0435)	-0.00493 (0.0493)	0.448 (1.302)	-0.281 (0.693)	0.119 (0.214)	-0.0310 (0.0620)	0.0461 (0.0610)	-0.0262 (0.0506)
Mean Control	0.380	0.332	0.455	6.233	27.42	1.297	-0.330	-0.0892	-0.0373
Observations	11,683	11,976	6,489	7,452	11,379	2,825	11,551	9,137	12,899
Mother has High School Education or Tertiary Education									
Eligible	0.0288 (0.0200)	-0.0279 (0.0337)	0.0493 (0.0283)	4.339 (1.533)	-0.329 (0.456)	-0.0751 (0.114)	-0.0484 (0.0442)	-0.0664 (0.0448)	-0.0319 (0.0410)
Mean	0.623	0.658	0.943	24.46	32.03	1.468	0.00357	0.226	0.291
Observations	19,432	13,387	14,748	21,484	14,422	6,585	16,431	14,728	14,178

Results from `rdrobust` (Calonico et al., 2014). All estimates include controls for mother's education, father's education and region. All columns contain instrumental variable estimates, where the discontinuity is used as an instrument for being eligible for a targeted voucher.

Table 1.7: School Choice and Educational Outcomes: Heterogeneous Effects by Distance to Nearest Private Voucher School with Add-ons that Joined the Policy

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Nearest P. Voucher School with Add-ons is less than 0.4 miles away									
Eligible	0.0140 (0.0331)	0.0187 (0.0556)	0.0244 (0.0347)	4.086 (2.092)	-0.256 (0.533)	0.0625 (0.132)	-0.0964 (0.0703)	-0.111 (0.0781)	-0.0310 (0.0656)
Mean Control	0.636	0.438	0.893	22.89	33.86	1.200	-0.127	0.153	0.176
Observations	6,969	5,543	8,488	9,280	8,520	4,933	7,136	5,121	5,855
Nearest P. Voucher School with Add-ons is more than 0.4 miles away									
Eligible	0.0533 (0.0373)	0.0482 (0.0421)	-0.0188 (0.0479)	2.519 (2.341)	0.213 (0.913)	-0.0180 (0.114)	0.0898 (0.0701)	0.0984 (0.0780)	-0.00310 (0.0642)
Mean Control	0.527	0.435	0.816	17.38	31.47	1.589	-0.179	0.0600	0.156
Observations	5,852	9,983	5,293	6,957	3,779	8,866	6,966	4,201	5,478

Results from `rdrobust` (Calonico et al., 2014). All estimates include controls for mother's education, father's education and region. All columns contain instrumental variable estimates, where the discontinuity is used as an instrument for being eligible for a targeted voucher.

Table 1.8: Conditional Independence Test

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Panel A: Below the cutoff without Controls									
Running Variable	-0.0014 (0.0035)	0.0141 (0.0051)	0.0238 (0.0049)	0.7383 (0.2525)	0.0170 (0.0767)	0.0282 (0.0183)	0.0135 (0.0073)	0.0037 (0.0071)	0.0173 (0.0066)
Mean	0.528	0.517	0.727	17.728	29.686	1.363	-0.143	0.112	0.145
Observations	25,728	24,991	24,950	25,600	25,325	10,208	23,451	21,377	21,420
Panel B: Below the cutoff with Controls									
Running Variable	-0.0063 (0.0032)	-0.0045 (0.0047)	-0.0012 (0.0039)	-0.0811 (0.2147)	-0.1372 (0.0647)	0.0413 (0.0182)	-0.0041 (0.0072)	-0.0110 (0.0071)	-0.0009 (0.0065)
Mean	0.528	0.517	0.727	17.728	29.686	1.363	-0.143	0.112	0.145
Observations	25,728	24,991	24,950	25,600	25,325	10,208	23,451	21,377	21,420
Panel C: Above the cutoff without Controls									
Running Variable	0.0332 (0.0009)	0.0404 (0.0013)	0.0776 (0.0012)	3.8766 (0.0835)	0.5601 (0.0181)	-0.0240 (0.0046)	0.0432 (0.0019)	0.0386 (0.0018)	0.0376 (0.0017)
Mean	0.619	0.640	0.967	26.958	31.937	1.304	0.004	0.229	0.260
Observations	58,450	56,792	56,713	58,207	57,682	22,123	54,885	51,044	51,260
Panel D: Above the cutoff with Controls									
Running Variable	0.0058 (0.0010)	0.0049 (0.0014)	0.0089 (0.0012)	0.1970 (0.0861)	0.0760 (0.0189)	-0.0217 (0.0053)	0.0085 (0.0022)	0.0085 (0.0021)	0.0058 (0.0020)
Mean	0.619	0.640	0.967	26.958	31.937	1.304	0.004	0.229	0.260
Observations	58,450	56,792	56,713	58,207	57,682	22,123	54,885	51,044	51,260

This table reports regression-based tests of the conditional independence assumption described in the text. Panels B and C show the coefficient on the running variable in models that control for mothers' education, fathers' education, household income, municipality of residence and child's gender. Estimates use only observations to the left or right of the cutoff as indicated in column headings. Robust standard errors are reported in parentheses.

Table 1.9: Conditional Independence Results

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Panel A: Below the cutoff									
Linear Reweighting	0.0021 (0.0039)	0.0015 (0.0054)	-0.0108 (0.0047)	1.6627 (0.2395)	-0.383 (0.0787)	0.0179 (0.0398)	-0.0186 (0.0088)	-0.0071 (0.0085)	-0.0046 (0.0077)
N untreated	58450	56792	56713	58207	57682	22123	54885	51044	51260
N treated	25728	24991	24950	25600	25325	10208	23451	21377	21420
Reweighting	-0.0103 (0.0042)	-0.0091 (0.0056)	-0.0293 (0.0052)	0.3893 (0.2405)	-0.5018 (0.0883)	0.0054 (0.0265)	-0.0281 (0.0091)	-0.0153 (0.0089)	-0.0103 (0.0078)
N untreated	58434	56774	56695	58191	57666	22095	54862	51031	51246
N treated	25726	24989	24948	25598	25323	10163	23450	21374	21417
Panel B: Above the cutoff									
Linear Reweighting	-0.0004 (0.0046)	0.0006 (0.0064)	0.0047 (0.0060)	1.0031 (0.4513)	-0.0230 (0.0927)	0.0017 (0.0362)	-0.0329 (0.0105)	-0.0108 (0.0109)	0.0033 (0.0101)
N untreated	58450	56792	56713	58207	57682	22123	54885	51044	51260
N treated	25728	24991	24950	25600	25325	10208	23451	21377	21420
Reweighting	-0.0010 (0.0049)	-0.0022 (0.0070)	-0.0036 (0.0072)	1.1981 (0.5473)	-0.1813 (0.1042)	0.0461 (0.0240)	-0.0339 (0.0111)	-0.0160 (0.0113)	-0.0002 (0.0109)
N untreated	58434	56774	56695	58191	57666	22095	54862	51031	51246
N treated	25726	24989	24948	25598	25323	10163	23450	21374	21417

This table reports CIA estimates of the effect of being eligible for targeted vouchers on school choice and educational outcomes. Panel A reports results for students below the cutoff and Panel B reports results for student below the cutoff. In each panel the first row reports results from a linear reweighting estimator, and the second row reports results from inverse propensity score weighting, as described in the text. Controls are the same as used to construct the test statistic reported in Table 1.8. Standard errors (shown in parentheses) were computed using a nonparametric bootstrap with 500 replications. The number of treated and untreated (above and below the cutoff) observations in the relevant outcome samples appear below standard errors.

Chapter 2

Loans for Whom and for What? Long-term Effects of Offering
Loans for Vocational vs College Education

I Introduction

In an era in which higher education costs and returns are both increasing, governments around the world are asking students to pay more for their education, often with the help of government-provided student loans. In the U.S, in particular, student loans have risen significantly since the mid-1990s. A similar trend is observed in many other developed countries ([Lochner and Monge-Naranjo, 2015](#)). Despite the increased interest in loans for higher education, we still know little about their causal impact on long-term outcomes.

The long-run impacts of these loan programs is unclear. In the short run, loans may increase initial enrollment ([Gurgand et al., 2011](#); [Solis, 2017](#)), allow students to focus more time on their academics and less time on financially supporting themselves¹, and choose majors, which can vary in price, with higher expected earnings. This could lead to higher graduation rates and labor-market returns. However, it could also lead to prolonged enrollments and higher expenditures. Moreover, loan programs may steer students to colleges that are not well matched to their academic ability. If students enroll in colleges that are “under matched” or “over matched” relative to the ability of other attending students, this mismatch could lead to increased drop out and lower earnings in the long run (see [Dillon and Smith, 2013, 2017](#) for a review of the literature on student and college match). These points emphasize the importance of quantifying the academic and earnings effects of these programs, and how they vary across the ability distribution.

This paper’s contribution is to show the long-term effects of two loan programs in Chile: one that provides students with loans for both universities and technical schools conditional on meeting a test score threshold; and one that gives students loans exclusively for technical schools conditional on meeting a GPA threshold. This setting offers a number of benefits. First, because loans for technical schools and loans for universities are discontinuously as-

¹[Keane and Wolpin \(2001\)](#), [Stinebrickner and Stinebrickner \(2008\)](#) and [Johnson \(2013\)](#) suggest that consumption can be quite low while in school for constrained youth. [Keane and Wolpin \(2001\)](#) and [Belley and Lochner \(2007\)](#) also suggest that constrained youth appear to work more than those who are not constrained.

signed based on a measure of academic performance, one can use a regression discontinuity design to estimate the causal effect of both programs. This is in itself valuable, as few studies overcome the endogeneity of credit access to provide causal estimates of the effects of loans.² Second, because both programs use different measures for assignment, there is substantial overlap between the two. In practice, this means that similar students are on the margin of getting access to either loans for just technical schools or loans for both universities and technical schools. This allows for a comparison of the effects of each.

Figure 2.1 describes how loans for technical schools and universities are assigned in Chile. Students who meet a test score threshold can access loans for both types of institutions. Students who are below this threshold can still get access to loans for technical schools if they meet a GPA threshold. These discontinuities allow me to examine the impact of: (i) having access to state loans for technical schools for students who do not have access to state loans, (ii) having access to state loans for universities for students who have access to state loans for technical schools, and (iii) having access to state loans for both technical schools and universities for students who do not have access to state loans. I use detailed student-level data for the entire population of students who participate in the higher education admission process, including information on students' enrollment and graduation, to estimate the impact of these programs on students' higher education access, choices, and graduation. Also, for the low-income students in my sample, I compare which program produces higher present-discounted expected earnings net of costs.

I find that loans have a large impact on students' probability of enrolling in higher education the year immediately after graduating from high school. Loans for technical schools increase short-run enrollment rates in 6 p.p., and loans for both universities and technical schools increase short-run enrollment rates in 13 p.p. These results are consistent

²Some notable exceptions include Solis (2017) who looks at the short run impacts of one of the programs analyzed in this study (loans for universities), as well as two contemporary papers by Montoya et al. (2017) and Bucarey et al. (2018) that look at the impact of one of the programs analyzed in this study (loans for universities) on earnings. In the U.S., Marx and Turner (2017) analyze the effect of loan offers on community college students' educational attainment.

with previous findings from [Gurgand et al. \(2011\)](#), who report positive effects of loan access on college enrollment in South Africa, and [Solis \(2017\)](#) who shows that loans for universities in Chile led to significant increases in higher education access. Nevertheless, these immediate enrollment impacts dissipate over time. Looking at long-term effects, I find that loans for technical schools increase students' probability of *ever enrolling* in tertiary education by just 1.4 p.p., and that loans for both technical schools and universities increase this probability by just 1.2 p.p. The latter has to do with the fact that all students in the sample, despite being low-performing and having little access to financial aid, have a high probability (roughly 90%) of ever enrolling in some type of tertiary education at baseline. Although a different setting, these results are in line with studies in the U.S. arguing that credit constraints are not a major impediment for higher education access.³

While loans have a small effect on the probability of ever enrolling in higher education, they do affect significantly which type of institution students attend. As expected, students who receive access to loans exclusively for technical schools substitute universities for technical schools. They decrease their probability of enrolling for the first time in a university in 2.3 p.p. and increase their probability of enrolling for the first time in a technical school in 3.7 p.p. Instead, students who already have access to loans for technical schools and are given access to loans for universities decrease their probability of enrolling in a technical school in 13.6 p.p. and increase their probability of enrolling in a university in 14.2 p.p. Likewise, students who do not have access to loans and are given access to both loans for technical schools and universities decrease their probability of enrolling in a technical school in 5.6 p.p. and increase their probability of enrolling in a university in 6.8 p.p.

Loans also affect the characteristics of the programs that students attend. Students who are given access to loans for universities, above loans for technical schools, enroll for the

³[Cameron and Heckman \(1998, 1999\)](#), [Keane and Wolpin \(2001\)](#), [Carneiro and Heckman \(2002\)](#), and [Cameron and Taber \(2004\)](#) all find little evidence that borrowing constraints affect college attendance. Instead, [Belley and Lochner \(2007\)](#) and [Lochner and Monge-Naranjo \(2011\)](#) argue that credit constraints may prevent some students from attending college. See [Lochner and Monge-Naranjo \(2012\)](#) for a more comprehensive review.

first time in degrees that last 0.2 years longer, with 14% higher expected tuition costs, that are 4.6 p.p. higher in terms of selectivity, and with 4% higher expected annual earnings. Likewise, students who are given access to both loans for technical schools and loans for universities enroll for the first time in degrees that last 0.2 years longer, with 12% higher tuition costs, that are 2.8 p.p. higher in terms of selectivity, and with 2.4% higher expected annual earnings. Instead, students who receive loans exclusively for technical schools enroll in degrees that are slightly longer, but that are not substantially different in terms of their cost, selectivity, or expected earnings.

Loans also allow students to spend less time on wage-earning activities and to devote more time to schooling. They increase students' probability of enrolling in a full-time program by between 3 and 5 p.p., they allow students to enroll in higher education sooner, and they reduce the number of gap years that students take while enrolled.

Looking at graduation rates up to nine years later, I observe that although giving students loans for universities and technical schools relative to loans only for technical schools allows students to enroll in degrees with 4% higher expected earnings, a substantial share of these students drop out or switch into lower quality alternatives. Due to the latter, students who have access to these loans increase the number of years they spend enrolled by 0.2 and the total amount spent on tuition by 1,076 USD while increasing their expected earnings by just 1%. Back of the envelope calculations suggest that on average these students are not substantially better compared to students who only have access to loans for technical degrees.

Results are even less encouraging when I focus on students who obtain access to both loans for technical schools and universities. This sample is composed of students with very low GPAs (below the 30th percentile) who gained access to both loans for technical schools and universities as a result of their performance on the standardized test. For these students, gaining access to loans decreases their probability of graduating from a technical school in 3 p.p., without significantly increasing their probability of graduating from a university.

This leads to a decrease in their probability of obtaining any degree of 2.2 p.p. Overall, the program increases the time that students spend enrolled in higher education by 0.26 years and the total amount that they spend on tuition by 960 USD, while at the same time decreasing their expected earnings by approximately 1%.

Giving low-performing students access to loans exclusively for technical schools is better in that it keeps students away from too expensive and academically demanding university alternatives. I find that these loans increase students' probability of graduating from a technical school by 2 p.p., without significantly reducing their probability of graduating from a university. This leads to an overall increase in graduation of 1.6 p.p. A major drawback, however, is that these students spend more time and money than we would have expected given their initial choices, suggesting that a number of students persist longer before dropping out. Due to the latter, compared to students who were not given access to loans, these students spend 0.15 additional years in higher education and 371 extra USD on tuition without seeing any significant increase in their expected incomes.

Although the regression discontinuity design only allows me to look at local effects, I combine the two measures of students' ability that I have (i.e. GPA and test score performance) to look at the effects of having access to loans for universities for students with varying GPA, and to look at the effect of having access to loans for technical schools for students with varying test score performance. Results from this analysis are in line with previous findings. Among students who are on the margin of having access to loans for universities, those with higher GPAs draw positive returns from getting access to these loans, while those with lower GPAs are ex-post worse off as a result of the program. Likewise, among students who are on the margin of getting access to loans for technical schools, those who reap the biggest benefits are relatively worse performing students.

This paper contributes to several aspects of the literature. It contributes to the scarce literature looking at the effects of loans on long-term outcomes. While some papers in the U.S. estimate the effect of loans on completed credits and transfer to four-year colleges,

there is no evidence on the long-term effects of loans on graduation outcomes and expected incomes. Existing studies in the U.S. include two observational studies by [Wiederspan \(2016\)](#) and [Dunlop \(2013\)](#), looking at the effects of restricting community college students' access to loans. There is also an experimental study by [Marx and Turner \(2017\)](#) that looks at the effect of giving community college students nonzero loan offers. In general, results point to a positive impact of loan access on completed credits and transfer to four-year colleges. However, this study raises a note for caution in interpreting previous findings. While loans may allow students to attend higher-quality alternatives and to complete more credits, this may not translate into better graduation outcomes and better expected incomes.

We can find more evidence on the long-term effects of loans in the international literature, where two contemporary papers by [Montoya et al. \(2017\)](#) and [Bucarey et al. \(2018\)](#) analyze the impact that loans for universities in Chile had on students' first years in the labor market. In line with findings from this study, the authors report that giving students access to loans for universities has no impact on students' short-run employment and earnings. This paper helps to understand why returns are so low. By looking at the characteristics of the degrees chosen by students, as well as the characteristics of the degrees where they are graduating from, I am able to show that while loans for universities allow low-performing students to attend higher quality programs, students have a hard time succeeding at graduating from these alternatives. Thus, it is not surprising that there is no effect on short-run earnings. Moreover, it is unlikely that there will be major positive effects on the long-run, given that loans are not helping students graduate from substantially better alternatives.

Furthermore, a question that remains open is whether it makes better sense to give these low-performing students access to loans for technical schools, as opposed to loans for universities. Results from this study help to answer this question. I show that loans for technical schools may be a better alternative for low-performing students in that they keep them from attending alternatives that are too expensive and academically demanding. Still, I am able to show that students who are given access to loans for technical schools spend

more time and money in higher education than students who do not receive access to loans. This is not a result of them choosing lengthier or more expensive alternatives, but rather of them persisting longer before dropping out. This points to another unintended consequence of offering students access to loans, which is that they may increase the amount of time and money that they spend without significantly increasing their future outcomes.

We can find more evidence on financial aid and higher education outcomes in the literature looking at grant aid, where studies typically find positive effects on college access for programs that have transparent eligibility criteria and straightforward application processes (see, among others, [Dynarski, 2003a](#), [Kane, 2003, 2007](#), and [Deming and Dynarski, 2009](#)), as well as positive effects on college persistence and completion (see [Dynarski, 2008](#); [Scott-Clayton, 2011](#); [Bettinger, 2004](#); [Bettinger et al., 2012a](#); [Castleman and Long, 2016](#)). Results from this study contrast with previous findings. However, this should come as no surprise considering how different loans are to grants. Loans provide money for college that needs to be repaid (see [Field, 2009](#) and [Rothstein and Rouse, 2011](#) for evidence on how students may respond differently to grants and loans), and they are given to below-average students, whereas grants are typically merit-based. Also, while grants typically place GPA or course load requirements on students, loans do not have major requirements for renewal (see [Scott-Clayton, 2011](#) for a discussion on the potential importance of such requirements).

This paper also contributes to the literature looking at student and college match. In particular, I show that steering low-performing students away from technical schools and into higher quality university alternatives can leave students worse off in terms of net returns. This implies that, in this setting, match quality ultimately matters more than absolute quality. These results contrast with finding from [Goodman et al. \(2017\)](#) and [Zimmerman \(2014\)](#) who find positive effects on college completion and labor market earnings of substituting two-year colleges with four-year colleges. Observational studies also typically find small effects of student and college match (see, among others, [Black et al. \(2005\)](#), [Bowen et al., 2009](#), [Mattern et al., 2010](#), [Chingos, 2012](#), and [Dillon and Smith, 2017](#)). Differences could

arise because in Chile, like many countries other than the U.S., students need to choose both a major and a university at the moment of applying. In such a setting, a mismatch could be particularly costly (a point emphasized by [Bordon and Fu, 2015](#), and [Malamud, 2010, 2011](#)), because students cannot easily respond to an overmatch by taking easier courses or switching majors ([Arcidiacono et al., 2014, 2016](#)).

These results have implications for policy. Although local in nature, results speak about the unintended consequences of offering low-performing students access to loans for universities; steaming from a potential mismatch between low-performing students and higher quality university alternatives. Nowadays, most countries offer students access to loans based solely on their financial need and regardless of their ability. Previous results, however, point to the potential benefits of offering, instead, different financial aid alternatives for students of different ability.⁴ Results also speak about the need to consider the benefits and costs of providing students loans, as many times loans can increase the amount of money that students spend without significantly increasing their expected outcomes. Results are also particularly relevant for the Chilean context, where a number of higher education reforms are now being considered.⁵

II Institutional Setting

The Chilean higher education system consists of 60 universities and 122 technical schools.⁶

Universities offer professional degrees that take on average 4 years to complete on time.

⁴It is hard to determine why students respond to university loans by switching to more demanding alternatives, if they have such a low probability of succeeding at these programs. Students may be fully informed and willing to trade lower probabilities of graduating, for a degree at a more prestigious institution. Alternatively, students may be poorly informed about the demands that different degrees place on them. An emerging literature suggests that students, particularly poor students, are misinformed about their educational prospects ([Hastings et al., 2016](#); [Hoxby and Turner, 2015](#); [Pallais, 2015a](#)).

⁵As of 2016, students in the lowest 50% of the income population, and regardless of their ability, can attend a number of universities and technical schools for free. Previous results suggest that such a policy could lead to mismatch for some low-performing students. Also, a reform to the loan program is currently being considered. Previous results speak about the appropriateness of a system that may induce some students towards technical schools while inducing others towards universities.

⁶These numbers, as well as the ones reported below, are all for the year 2008. While there is some variation throughout the years, the numbers provide a good description of the higher education system.

Technical schools offer both professional degrees that take 4 years to complete on time, and technical degrees that take 2 to 3 years to complete on time. One of the differences between universities and technical schools is that, while universities can offer all type of professional degrees, technical schools can only offer certain professional degrees that do not require a previous bachelor's degree.⁷ All of the 60 universities are non-for profit, with 16 of them being public and 44 being private. Instead, most of the technical schools are for profit.

Table 2.1 describes the main characteristics of degrees offered by universities and technical schools in 2008. As can be seen, in 2008, approximately 68% of students enrolled in higher education were attending a university, and 32% were attending a technical school. Technical degrees take on average 2.5 years to complete on time, have a total cost of approximately 3,867 USD, and are in the 37th percentile in terms of selectivity⁸. Its graduates have expected employment rates of 66% one year after graduation, and expected annual incomes of approximately 12,898 USD four years after graduation.⁹ Instead, professional degrees offered at technical schools last longer, 4 years; have higher expected costs, 9,209 USD; but are also higher in terms of selectivity, 43rd percentile; expected employment, 71%; and expected incomes, 15,152 USD. Professional degrees offered by universities also last 4 years on average. However, they are more expensive, 14,982 USD; more selective, 74th percentile; and have higher expected employment rates, 78%, and earnings, 20,735 USD.

How the admission process works is that students first need to sign up for a standardized college admission test that is given at the end of the academic calendar year, in December. If students want to receive state financial aid they also need to complete a socioeconomic

⁷Professional degrees offered by technical schools include, for example, education, social service, risk prevention, design, accountant, informatics, or mechanics.

⁸Selectivity is measured by the average performance on the math and language test of students who enroll for the first time in that field (CINE-UNESCO category) and institution in the 2008 to 2016 period. Field/institution combinations are ordered from least to most selective to get percentile selectivity. Each degree is imputed the percentile selectivity of the students enrolled in that field/institution. More information can be found on section III.

⁹Expected employment and expected income are estimated based on expected measures reported by the Ministry of Education. An average measure of expected income and employment is computed for each field (CINE-UNESCO category)/institution combination. Each degree is imputed an expected income and employment based on the expected measures for that field/institution. More information can be found on section III.

verification form. In December, all students take the standardized college admission test, which includes mandatory exams in math and language and optional tests in science and history. Scores for these tests are scaled to a distribution with range 150 to 850 and a mean and median of 500. A few weeks after taking the standardized test, students receive their results. Information from the socioeconomic verification form, along with their performance on the standardized admission test, and their GPA, determines students' access to state financial aid, so at this point students know whether they are eligible for each type of aid. Students, then, have to enroll in their chosen institution and degree.

As in many other postsecondary education systems, though typically not in the U.S., students need to choose both an institution and a career, for example, Economics at Columbia University. Admission to university degrees depends on students' performance on the standardized college admission test and their high school GPA. In fact, 25 of the 60 universities participate in a centralized admission system, where students need to submit an application with up to 8 ranked choices, and are then assigned to each degree based on their composite score (which is a combination of their test results and GPA) until all slots are filled or demand is satiated. Technical schools, instead, typically do not have any type of academic requirement for admission.¹⁰

Each degree has a different tuition price that can vary depending on the institution and career. In order to finance their studies, students can request a loan in the private banking system or apply for state financial aid. Universities and technical schools sometimes offer grants to attract very high performing students, but these are a minor component of financial aid. In practice, students have access to four main public sources of funding: (i) income contingent loans for universities, (ii) state-guaranteed loans for universities, (iii) state grants for universities, and (iv) state-guaranteed loans for technical schools.

As was mentioned above, access to financial aid depends on the information that students provide in the socioeconomic status verification form, their performance on the standardized

¹⁰In a few cases technical schools require students to give a test for admission.

college admission test, their GPA, and the type of institution that they choose. For instance, students who belong to the lowest 80% of the income population, and whose average score in the language and math exam is above 475 (40th percentile), can access income contingent loans if they enroll in one of 25 traditional universities, or state-guaranteed loans if they enroll in one of 21 non-traditional accredited universities or one of 21 accredited technical schools. Students who belong to the lowest 80% of the income population, and score below 475, can still get access to state-guaranteed loans for technical schools if their GPA is above 5.3 (30th percentile) and they enroll in one of 21 accredited technical schools. Grants, instead, are only available for low-income students, who are in the lowest 40% of the income population, score above 550 in the standardized test (70th percentile), and who enroll in one of 25 traditional universities. In this paper, I focus on the effect of gaining access to state-guaranteed loans for technical schools and state loans for universities for students who score below 550, and are, therefore, non-eligible for state grants.

State loans allow students to borrow money up to what is called the referential tuition, which is a price set by the Ministry of Education of how much a program should cost depending on the institution's assets, quality, and labor market perspective of its graduates. On average, referential tuitions are set around an 85% of actual tuition costs for universities and around a 90% of actual tuition costs for technical schools. This means that students need to cover a 15% or 5% of the tuition costs themselves if they attend a university or technical school, respectively. Students also need to cover living expenses and other costs associated with attending college (e.g. books and transportation).

State loans can be renewed each year, without a need to reapply. Students can finance their degree for up to 3 years in excess of the official duration for university degrees, 2 years in excess for professional degrees at technical schools, and 1 year in excess for technical degrees. Although universities and technical schools are allowed to put some conditions for program renewal¹¹, in practice, most institutions require students to simply maintain their

¹¹This had to do, in part, with the fact that the program makes higher education institutions responsible for student default if the students drop out.

regular student status. Students are allowed to change institution or degree once.

In terms of their interest rate and enforceability of repayments, state-guaranteed loans and income contingent loans differ in several important ways. Income contingent loans are managed by universities, they have a real interest rate of 2% per year with a maximum of 15 years of payment (after which the loan repayment is written off), the repayment starts two years after the student's graduation, and the installments corresponds to 5% of the borrower's income. Instead, state-guaranteed loans are very similar to other loans available in the conventional market. Up to 2011, state-guaranteed loans had a real interest rate of 6% per year, which was slightly higher than the average mortgage rate; the repayment started 18 months after the student's graduation; and students needed to make fixed payments over a period of 5, 10, or 15 years, depending on the total debt. Importantly, private banks are in charge of the whole process for state-guaranteed loans, meaning that they are entitled to use all available legal mechanisms to recover the debt, including the release of information to credit score institutions, asset impoundment, and judicial collection. As a point of reference, in 2017, of the total number of students holding state-guaranteed loans who were in payment period, approximately 40% were in default.¹²

State-guaranteed loans were first implemented in 2006. Since then, the number of students holding loans has increased significantly, from approximately 16 thousand in 2006 to 650 thousand in 2016. The high level of student debt and the difficulty that many students face repaying their loans, led to massive student protests in 2011. Since then, higher education in Chile has been at the center of the public debate, and the government has implemented multiple reforms. Nowadays, students in the lowest 50% of the income population can attend 30 universities and 13 technical schools for free. State-guaranteed loans are still available for higher-income students. However, the government is currently considering a complete reform of state-guaranteed loans.

¹²Source: *Cuenta Publica Comisión Ingresos* 2017.

III Data and Descriptive Statistics

In the analysis, I focus on students who completed high school between 2007 and 2009 and who signed up for the standardized college admission on the year they graduated from high school. State-guaranteed loans were first offered in Chile in 2006. However, because I only have data on students' socioeconomic classification as of 2007, I do not use information from previous cohorts. Still, focusing on students who graduated from high school between 2007 and 2009, allows me to observe students' enrollment and graduation outcomes between 7 and 9 years after high school graduation.

In order to characterize students' socioeconomic status, I use data from the registry of students who sign up for the standardized college admission test. This data set contains information on students' test scores, high school GPA, as well as a rich set of socioeconomic characteristics, such as self-reported family income, parental education, parental work status, and school of graduation. I complement this data with information from students' socioeconomic verification form (FUAS) to determine whether students in my sample are eligible for state financial aid.

To determine students' enrollment and graduation patterns I use data from administrative files from the Ministry of Education that capture enrollment and graduation from all higher education institutions in the country. These datasets allow me to determine where a student enrolled, as well as whether he graduated in the 2008 to 2016 period. Because I have information for all universities and technical schools in the system, and very few people leave the country to study undergraduate programs abroad, I do not need to worry about a miss-classification of enrollment paths, which is a typical problem encountered by studies looking at higher education outcomes in other countries.

To characterize degrees, I use data from the Ministry of Education providing descriptive information for each of the technical and professional degrees being offered in the 2008 to 2016 period. This dataset allows me to characterize degrees based on their price, length, and field of study (CINE-UNESCO category). To measure degree selectivity, I use information

on test score performance for all students who enroll for the first time in each field/institution combination in the 2008 to 2016 period. I order field/institution combinations from least to most selective, and impute the selectivity percentile for each degree based on the selectivity percentile for that field/institution combination. Likewise, to measure expected graduation rates, I compute the average graduation rates for all students who enrolled for the first time in that field/institution combination in 2007.¹³

In order to get a sense of the returns to pursuing different degrees, I complement the above dataset with information from the Ministry of Education on average earnings and employment rates for students who graduated from each degree in the 2000 to 2015 period.¹⁴ Employment measures are computed based on average employment rates, one year after graduation, for students who graduated from a given degree. An individual is considered to be employment if he has a monthly salary above the minimum wage. Income measures are computed based on average monthly salaries four years after graduation. Likewise, the government only considers monthly salaries above the minimum wage.¹⁵ In practice, because I do not have information on employment and earnings for all of the existing degrees, I assign a measure of expected employment and income for each degree based on the average expected employment and earnings for degrees in that field/institution.

Table 2.2 characterizes the students in the three sub-samples analyzed and shows how these samples compare to the general population of high school graduates who signed up for the standardized admission test in the 2007 to 2009 period. Of the total amount of high school graduates, 50% completed the socioeconomic verification form and were classified as belonging to the lowest 80% of the income population. The samples used in the analysis are composed of students who met the socioeconomic requirement for state loans and who: (i) scored below 475 on the standardized admission test (i.e. they were non-eligible for state

¹³I measure whether or not students have graduated up to 10 years later, regardless of where they have graduated from.

¹⁴In practice, the Ministry of Education uses information from students who graduated in 2000, 2001, 2005, 2006, 2007, 2008, 2009, 2010, 2011, 2012, 2013, 2014, and 2015.

¹⁵The ministry of Education reports monthly income ranges for each degree. I assign the mid-income for each degree.

loans for universities) and were on the margin of getting state loans for technical schools, (ii) had a GPA above 5.3 (i.e. they were eligible for state loans for technical schools) and were on the margin of getting loans for universities, and (iii) had a GPA below 5.3 (i.e. they were non-eligible for state loans for technical schools) and were on the margin of getting access to both state loans for technical schools and state loans for universities.

As can be seen in Table 2.2, the sample of high school graduates is composed of 584,759 students. Half of these students are women, they have on average 18 years at the moment of applying, and about 40% live in the capital. Among these students, 43% graduated from a public school, 48% graduated from a private subsidized school and just 10% graduated from a private school.¹⁶ Their household characteristics show that these students have on average 4.5 household members, of whom 1.2 work. About 60% have the father as the head of household and 30% have the mother as the head of household. The annual income reported for these households is 8,859 USD. About 25% of students have mothers with tertiary education, and 30% have fathers with tertiary education. Most households have a father that works full-time (63%), but just one third have a mother that works full-time (33%). In terms of academic performance, students have on average a GPA of 5.6 and obtain approximately 490 points on the standardized language and math test.

Data on enrollment and graduation records shows that 82% of students who graduated from high school and signed up for the standardized admission test enroll in some type of tertiary education; with 32% enrolling for the first time in a technical school and 46% enrolling for the first time in a university. Seven to nine years after high school graduation, 42% of students have graduated from any degree, representing approximately half of higher education enrollees.

Students in the three sub-samples analyzed come from a lower socioeconomic background; which is not surprising considering that these students qualify for state financial aid. Most of these students attended a public or private subsidized school and very few of them attended

¹⁶Chile has a universal voucher system since the 1980s.

a private school. They come from bigger households, where fewer people work, report lower annual incomes and have less educated parents. Still, enrollment rates are high in the three subsamples analyzed. Seven to nine years later, between 40 to 60% of students have graduated from any degree, representing between a 45 to 60% of higher education enrollees.

Now, among these students, those who do not have access to loans and are on the margin of getting access to loans for technical schools are on average students that perform badly in the test (405), and whose GPA is close to 5.3. As can be seen in Table 2.2, these are students whose socioeconomic status is below that of students in the other two sub-samples analyzed. These students have a higher chance of enrolling in a technical school as opposed to a university. Instead, students who do not have access to loans and are on the margin of getting access to loans for both technical schools and universities are students who, compared to the previous group, have relatively low GPAs (5.2), but perform better on the standardized test (466). These students have a higher socioeconomic status compared to students in the other two sub-samples analyzed. Still, these students have a higher probability of enrolling in a technical school as opposed to a university, compared to students who have higher GPAs, and who are on the margin of getting access to loans for universities on top of loans for technical schools.

IV Empirical Strategy

In this study, I exploit the fact that, conditional on meeting the socioeconomic requirements, access to state-guaranteed loans and income contingent loans for universities is a discontinuous function of students' performance on the standardized college admission test, and access to state-guaranteed loans for technical schools is a discontinuous function of students' GPA. The latter can be better seen in Figure 2.1, which shows how students who cross the test score threshold get access to both loans for universities and technical schools. It also points out how students who are below this threshold can still get access to loans for technical

schools if they cross a GPA threshold.

The aforementioned allows me to implement a regression discontinuity design along three different margins to look at the impact of: (i) having access to state loans for technical schools for students who do not have access to state loans, (ii) having access to state loans for universities for students who have access to state loans for technical schools, and (iii) having access to state loans for both technical schools and universities for students who do not have access to state loans. Figure 2.2 shows that there are enough observations in the relevant period, to exploit these three margins separately.

In each case, I estimate the effect of crossing the threshold on an individual's educational outcomes by using the following equation:

$$y_i = \alpha_1 s_i + \alpha_2 s_i 1(s_i \geq 0) + \lambda 1(s_i \geq 0) + \alpha_3 X_i + \gamma_c + \epsilon_i \quad (2.1)$$

where y_i is the outcome of interest, for example, enrollment in a university for individual i , s_i is the difference between i 's score and the cutoff score, $1(s_i \geq 0)$ is an indicator variable equal to one if i 's score is above the cutoff score, X_i are socioeconomic characteristics, γ_c are high school class fixed effects, and ϵ_i is an error term. I include high school class fixed effects and socioeconomic characteristics simply to gain precision. Depending on whether I am looking at the effect of accessing loans for technical schools or loans for universities, s_i may equal a student's GPA distance from the cutoff or test score distance from the cutoff. In the latter case, I use students' first test score results, to avoid potential endogeneity driven by students retaking the test.

I estimate equation 2.1 using data within a narrow score window around the cutoff point. In practice, throughout the analysis I use a bandwidth of 0.6 points when looking at the effect of gaining access to loans for technical schools, 65 points when looking at the effect of gaining access to loans for universities above loans for technical schools, and 70 points when looking at the effect of gaining access to both loans for technical schools and universities. These bandwidths correspond closely to the optimal bandwidths suggested by Imbens and

Kalyanaraman (2012a). Such optimal bandwidths vary by outcome. However, I choose to fix the bandwidths across regressions to have a single sample in each case. Estimates remain the same when using alternative bandwidths, polynomials of the running variable, and standard errors that are clustered by the running variable (see Appendix 2.A).¹⁷

In order to estimate the impact of gaining access to loans for technical schools, for students who do not have access to state loans, I restrict my sample to students scoring below 475 on the standardized test. Likewise, in order to estimate the impact of gaining access to loans for both technical schools and universities, for students who do or do not have access to loans for technical schools, I restrict my sample to students whose GPA is either below or above 5.3. Because GPAs are given at the moment of taking the standardized test, restricting the sample to students whose GPA is above or below 5.3 is of no concern. However, restricting the sample to students whose standardized test is below 475 could potentially lead to biases. In particular, if individuals whose GPA is below 5.3, and who are non-eligible for loans for technical degrees, have a higher probability of taking the standardized admission test, then this could bias the results. In practice, however, crossing the GPA threshold has no effect on students' probability of taking the standardized test the year immediately after graduating from high school, nor does it affect the probability of scoring above 475 on the standardized admission test the year immediately after graduating from high school.

For students who do not have access to state loans and are on the margin of getting access to loans for technical schools, equation 2.1 gives me an estimate of the impact of ever getting access to these types of loans. That is, because students in the control group who do not meet the GPA requirements will never meet those requirements. Nevertheless, it is worth highlighting that these students can retake the standardized college admission test and eventually cross the 475 threshold. In practice, approximately 10% of students in this sub-sample eventually become eligible for loans for both universities and technical schools. Crossing the GPA threshold, therefore, increases the probability of gaining access to loans

¹⁷Including standard errors that are clustered by the running variable is particularly relevant when using the GPA as the running variable, since it is more discrete.

for technical schools, the year immediately after graduating from high school by 100 p.p., and it increases the probability of ever gaining access to loans for technical schools by 89 p.p.¹⁸

Instead, for students who are on the margin of getting access to loans for university degrees, equation 2.1 gives me an estimate of the effect of being eligible for loans on the year immediately after graduating from high school. In this case, students in the control group can retake the standardized test and eventually become eligible. In fact, approximately 30% of students in the control group eventually become eligible for loans for universities and technical degrees. The RD, therefore, does not measure the effect of ever being eligible, but rather the effect of being eligible from the start.

V Results

A Balancing Checks

I begin by performing standard balancing checks to examine whether individuals just above and just below the cutoff in each of the three sub-samples analyzed are similar in terms of their observable characteristics. I look at a set of socioeconomic variables that should not be affected by the program. If the procedure is valid then RD estimates should be equal to zero.

We can see the results on Table 2.3. I look at students' gender, age, whether they attended high school in the country capital, the type of school where they completed high school, number of individuals in their household, number of individuals in their household who work, number of individuals in their household attending tertiary education, who the head of household students report to be, household annual income in 2017 USD, parents' educational level, and whether parents work full or part-time.

¹⁸Being above the GPA threshold for loans for technical schools decreases the likelihood of ever becoming eligible for loans for both universities and technical schools in approximately 1.5 p.p.

In each of the three sub-samples analyzed, I observe that students who are close to the cutoff are similar in terms of their socioeconomic characteristics. Coefficients are small in magnitude and precisely estimated, indicating that students are very similar to each other. I do observe some significant outcomes. However, in each sub-sample, there are at most 3 significant outcomes at the 10% level and one significant outcome at the 5% level. More importantly, all of the coefficients are small in magnitude, indicating that students close to the cutoff, in each of the three cases, are comparable in terms of their baseline characteristics.

To further check whether there is any sign of score manipulation I proceed to look at whether there is any evidence of a visible jump in the density around the discontinuity for loans. Figure 2.3 shows histograms of scores relative to cutoff value. There is no evidence of bunching on the test score distribution for students who are below or above the 5.3 GPA threshold. In fact, tests by McCrary (2008) and Cattaneo et al. (2016b) fail to reject the null hypothesis of equal densities around the cutoff (see Appendix 2.B). Analyzing GPA distribution is somewhat more complicated, because the data is more discrete. Figures 2.3, however, shows no evidence of bunching. Also, a test by Frandsen (2017) for discrete running variables fails to reject the null hypothesis of no manipulation.¹⁹

B Loans and Higher Education Choices

In this section, I analyze the impact that loans for universities and/or technical schools have on higher education enrollment. I begin by looking at the impact that loans have on students' probability of ever enrolling in higher education. To the extent that students are credit constrained, loans for universities and/or technical schools could increase higher education access.

Results looking at students' probability of ever enrolling in higher education can be found in Figure 2.4 and Table 2.4. Figure 2.4 plots the probability of enrolling in tertiary education as a function of the standardized GPA and test score. Black lines depict a linear fit for control

¹⁹Manipulations test: pvalue=0.403 (k=0.02), p-value=0.134 (k=0.01), p-value=0.044 (k=0.0)

and treatment units separately and grey dots represent the sample average for each disjoint bin. Figure 2.4 (a), (b) and (c) show that students above the cutoff score for loans for universities and/or technical schools have a slightly higher probability than students below the cutoff of attending any type of tertiary education. However, all of the effects are small in magnitude mainly because students below the cutoff, in each of the three sub-samples, already have a relatively high probability of ever enrolling in some type of tertiary education.

Estimates from Table 2.4 confirm the visual analysis. Students who do not have access to state loans, and are on the margin of getting access to loans for technical schools, have a baseline probability of enrolling in some type of tertiary education of 87%. For these students, gaining access to loans for technical schools increases their probability of ever enrolling in some type of tertiary education by 1.4 p.p. Likewise, students who have access to loans for technical schools, and are on the margin of getting access to loans for universities, have a baseline probability of enrolling in some type of tertiary education of 95%. Having access to loans for universities increases their probability of enrolling in some type of tertiary education by 0.6 p.p. Results for students who do not have access to state loans and are on the margin of getting access to both loans for universities and technical schools follow a similar pattern. Here students in the baseline group have a probability of ever enrolling in some type of tertiary education of 94%. For these students, gaining access to loans increases their probability of ever enrolling in some type of tertiary education by 1.2 p.p. Although significant, previous coefficients are all small in magnitude. This suggests that in the long-run credit constraints are not a real impediment for higher education access, at least for students on the margin of getting access to loans.

I next look at the effect that loans for universities and/or technical schools have on students' probability of choosing: a technical degree at a technical school, a professional degree at a technical school, or a professional degree at a university. Despite not having a major effect on higher education access, loans could still affect where students choose to go. Depending on whether different degrees represent close substitutes for one another, loans

could steer students towards technical schools or universities.

Figure 2.4 and Table 2.4 analyze where students enroll for the first time. Figure 2.4 (a) shows that compared to students who do not have access to loans, students who are above the cutoff for loans for technical schools have a higher probability of enrolling in a professional degree at a technical school. It also shows that they have a lower probability of enrolling in a university. This indicates that, in fact, loans for technical schools steer students away from universities and into technical schools. Results in Table 2.4 confirm the visual analysis, having access to loans for technical schools increases students' probability of enrolling for the first time in a professional degree at a technical school by 4.5 p.p. (a 31% increase). Additionally, it decreases students' probability of enrolling for the first time in a university by 2.3 p.p. (a 10% decrease). Interestingly, there is no significant change on the probability of enrolling in a technical degree; suggesting that students substitute university degrees for professional degrees at technical schools, but that they are unwilling to substitute university degrees for technical degrees.

Giving students access to loans for university degrees steers students in the opposite direction. Figure 2.4 (b) shows that for students who have access to loans for technical schools, gaining access to loans for universities decreases their probability of enrolling in a technical or professional degree at a technical school. It also increases their probability of enrolling in a university. Likewise, for students who do not have access to loans, having access to loans for both technical schools and universities decreases their probability of enrolling in a technical degree at a technical school, and it increases their probability of enrolling in a university (see Figure 2.4 (c)). Results in Table 2.4 confirm previous findings and show that for students who already have access to loans for technical schools, gaining access to loans for universities decreases their probability of enrolling in a technical school by 13.6 p.p. (a 25% decrease), and it increases their probability of enrolling in a university by 14.2 p.p. (a 36% increase). For students who do not have access to loans for technical schools, gaining access to both types of loans decreases their probability of enrolling in a technical degree at

a technical school by 6.5 p.p. (a 17% decrease), and it increases their probability of enrolling in a university degree by 6.8 p.p. (a 17% increase).

Because students can only get access to loans if they enroll in an accredited university or technical school, Table 2.4 further analyzes whether having access to loans switches students away from non-accredited and into accredited institutions. Results indicate that 10 to 15% of students in the three sub-samples analyzed enroll in a non-accredited institution. Having access to any type of state loan decreases their probability of enrolling in a non-accredited institution between 1.5 and 4 p.p.

Previous results analyze how loans affect the type of degree that students choose. To get a better sense of the specific degrees that these students are attending Table 2.5 describes the most common technical degrees, professional degrees at technical schools and professional degrees at universities attended by students in the control group in each of the three subsamples analyzed. Technical degrees attended by these students include, for example, nursery, risk prevention, gastronomy, mechanic and business administration. Professional degrees also include nursery, risk prevention and business, as well as other degrees in the field of education.

To further understand how changes in students' choices ultimately affect the characteristics of the degrees attended by students, Table 2.6 looks at the impact that each program has on the characteristics of the degrees where students enroll for the first time, including its: average length, measured by the number of years it takes to complete the degree on time; annual tuition cost; total tuition cost; percentile selectivity; field of study, where fields are grouped into a non-STEM, STEM, business & administration, and health area²⁰; expected graduation rates; expected employment rates one year after graduation; and expected earnings four years after graduation.²¹

²⁰Field categories are based on CINE-UNESCO categories. I group agriculture, science and technology into a STEM area; and art and architecture, social science, law, education and humanities into a Non-STEM area.

²¹Average length, annual tuition, total tuition, field of study and expected graduation all equal zero if the individual never enrolls in higher education. Expected employment rates and expected annual earnings equal 0.55 and 8,844 USD for students who never enroll. These numbers correspond to the average employment

Table 2.6 shows that although loans for technical schools switch students away from universities and into technical schools, in practice they do not have a major effect on the characteristics of the degrees that students attend. In fact, results show that compared to students who do not have access to loans, students who have access to loans for technical schools attend degrees that are slightly longer (0.08 extra years), but that are not substantially different in terms of their annual cost, total cost, selectivity, field of study, expected graduation rates, expected employment, or expected annual earnings. There are a few significant estimates which is partly driven by the fact that we are looking at non-conditional estimates and loans for technical schools have a slight positive impact on the probability of ever enrolling in higher education.

Instead, loans for universities allow students to attend degrees that last longer, are more expensive, more selective, and have higher expected employment rates and earnings. Table 2.6 shows that students who have access to loans for universities, above loans for technical degrees, choose degrees that last 0.2 extra years (a 6% increase), that have an extra cost of 1,478 USD (a 14% increase), and that are 4.6 p.p. higher in terms of selectivity (a 9% increase). Loans also increase the probability that students will enroll in non-STEM fields and decrease the probability that they will enroll in business and administration or health fields. In terms of expected graduation rates and earnings, loans allow students to enroll in degrees with slightly higher expected graduation rates, that have 1.3 p.p. higher expected employment rates (a 2% increase), and that have 653 USD higher expected annual earnings (a 4% increase).

Similarly, giving students access to state loans for both universities and technical schools above no loans allows students to attend degrees that last 0.219 extra years (a 6% increase), that have an extra cost of 1,248 USD (a 12% increase), and that are 2.8 p.p. higher in terms of selectivity (a 6% increase). These students are also more likely to enroll in a degree in a non-STEM field. In terms of the degrees future prospects, students who gain access to loans

rates and annual earnings for individuals who completed high school education and were 30 to 35 years old in 2017 (Casen 2017).

for technical schools and universities are attending degrees with slightly higher expected graduation rates and that have extra annual earnings of 349 USD (a 2.4% increase).

C Loans, financial aid, and study time

In this section I analyzing the extent to which loans help to alleviate the costs associated with attending higher education. I look at how much money students in the treatment and control group spend on tuition the first year they enroll in higher education, how much they receive on grant aid, how much they are allowed to borrow in loans²², and how this ultimately affects the out-of-pocket amount that students face on that first year. The out-of-pocket amount equals the amount that students need to pay on tuition, but it does not consider other living expenses or other costs associated with attending higher education. I then analyze how loans affect the time that students devote to schooling. I focus on the probability that students enroll full-time, their probability of enrolling on time (i.e., immediately after graduating from high school), the number of degrees where they enroll, and the number of gap years they take while enrolled. All of these variables equal zero if an individual never enrolls in higher education.

As can be seen in Table 2.7, and in line with what was reported in section B, students in the control group spend between 2,200 and 3,000 USD on tuition their first year enrolled. Because these are relatively low-performing students, they have little access to state grant aid. Still, students who belong to the first two income quintiles and have a GPA above 5.0, are eligible for approximately 800 USD in grant aid if they enroll in an accredited technical degree (*Beca Nuevo Milenio*). Likewise, students who graduate from a subsidized school and are in the top 5% of their high school class, are eligible for approximately 800 USD in grant aid if they enroll in an accredited technical school and 1800 USD in grant aid if they enroll in an accredited university (*Beca Excelencia Academica*).²³ As can be seen in

²²I assume that students can borrow a 90% of tuition costs if they enroll in an accredited technical school and 85% of tuition costs if they enroll in an accredited university. These correspond to the average amounts that referential tuitions cover for technical schools and universities.

²³Other forms of grant aid include: (i) grants given to students who belong to the first two income quintiles,

Table 2.7, students in the control group receive on average between 200 and 400 USD in grant aid. Having access to loans slightly decreases the amount that these students receive in grant aid, which is mainly a consequence of students migrating from technical degrees, which makes them ineligible for the *Beca Nuevo Milenio*. Still, the coefficients are small in magnitude, indicating that on average students who receive loans for technical schools receive 88 USD less in grant aid; that students who receive loans for universities, on top of loans for technical schools, receive 1 USD less in grant aid; and that students who receive loans for both technical schools and universities, receive 29 USD less in grant aid.

Table 2.7 next compares the amount that students in the treatment and control group may access in terms of loans. As was described in section IV, students who do not meet the test score requirements for loans for technical schools and universities, can retake the test and eventually become eligible. Because of the latter, students in the baseline group may be eligible for loans on their first year enrolled if, for instance, they choose to retake the test and delay their higher education enrollment. Results in Table 2.7 show that students who did not have access to loans on the year immediately after graduating from high school, and who were on the margin of getting access to loans for technical schools, accessed on average 230 USD in loans on their first year enrolled, compared to 1,213 USD for students who were above the GPA threshold for loans. Students who did not have access to loans the year immediately after graduating from high school, and who were on the margin of getting access to both loans for technical schools and universities, accessed on average 536 USD in loans on their first year enrolled, compared to 2,344 USD for students who were above the test score threshold for loans. Instead, students who were on the margin of getting access to loans for universities, are students who already had access to loans for technical schools. Here, students in the baseline group accessed 1,552 USD in loans. Still, crossing the test score threshold increased the amount that these students could borrow in 798 USD. Giving

score above 550 in the standardized test, and enroll in a traditional university (*Beca Bicentenario*); (ii) grants given to students who belong to the first two income quintiles, graduate from a subsidized school, score above 640 on the standardized test, and enroll in an accredited institution (*Beca Juan Gomez Milla*). Students in my sample, however, do not qualify for these forms of aid.

students access to loans has an important effect on the out-of-pocket amount that students must pay on their first year in college. This can be seen in Table 2.7, which shows how students who have access to loans right after graduating from high school face between 600 and 1,600 less USD in terms of out-of-pocket tuition costs.

Having noted the extent to which loans help to alleviate the costs that students face in their first year enrolled in higher education, I proceed to look at how this affects the time that students may devote to their studies. I begin by looking at the probability that students enroll as full-time students. On average, 70% of students in the control group enroll full-time. Students who cross the threshold for loans are 0.3 to 0.5 p.p. more likely to enrolled full-time. Students are also more likely to enroll in higher education sooner. While roughly 50% of students in the baseline group enroll in higher education the year immediately after graduating from high school, this number is 6 to 13 p.p. higher for students who have access to loans. In terms of the number of degrees where students enroll, I observe that students on average enroll in approximately 1.5 degrees. Having access to loans, however, seems to have no major effect on the number of degrees where students enroll. Finally, I look at the number of gap years that students take while enrolled, where I define gap years as the number of years that students spend out of the higher education system while enrolled.²⁴ Results show that students in the baseline groups take approximately 0.5 gap years while enrolled. While loans for universities have no major effect on the number of gap years, I do observe that students who have access to loans for technical degrees take 0.04 fewer gap years compared to students who do not have access to loans.

D Loans and higher education graduation

In this section, I look at how each of the programs analyzed affects students' long-term educational outcomes. I begin by looking at the effect on students' probability of graduating from a: technical degree, a professional degree at a technical school, a university degree, or

²⁴This number does not consider the number of years that students may have taken before enrolling in higher education for the first time. It also does not consider years after dropout.

any degree. A priori, it is unclear how loans may affect higher education graduation rates. Loans help to alleviate credit constraints, and, as seen in section C, can potentially allow students to devote less time to wage-earning activities and more time to schooling. However, it is not obvious whether this could impact students' graduation. More importantly, as seen in section B, loans steer students into different types of programs. The impact that this may have on higher education graduation rates will depend on whether these new alternatives represent a better or worse match.

I complement previous analysis with estimates of how loans affect the costs and expected benefits for students, to get a sense of the potential returns to each program. To measure costs, I estimate the impact that each program has on the number of years that students spend enrolled and the total amount of money that they spend on tuition. Here, I make the distinction between what we would have expected students to spend given their initial choices and how much they actually have spent 7 to 9 years after high school graduation. I make this distinction to get a sense of how much of the increased costs are a result of students choosing more expensive or lengthier higher education alternatives, and how much of it is a result of students spending more time enrolled in their chosen degrees. To measure expected benefits, I use information on expected employment rates and expected earnings for the degrees chosen by students. Here too, I make the distinction between what the expected benefits would have been, had students graduated from the degrees that they chose in the first place, and how much the expected benefits are given where students have actually graduated from 7 to 9 years after high school graduation. I assume an expected employment rate of 0.55 and expected annual earning of 8,844 USD for students who do not graduate from any degree, or who never enroll in higher education. These numbers correspond to the average employment rates and annual earnings for individuals who completed high school education and were 30 to 35 years old in 2017.²⁵ Making the distinction between what the expected benefits would have been had students graduated from their initial choices, and

²⁵These estimates are based on data from the Chilean household survey for 2017, *Casen 2017*. I consider individuals to be employed if they have a monthly wage above the minimum wage.

what they are given where students actually graduate from, allows me to determine whether changes in expected benefits are a result of students' initial choices or graduation patterns.

To compare the costs and expected benefits of each alternative, I perform a rough estimate of what the present value is for each student. To do so, I compute the present value of what the individuals' expected earnings are, minus what they would have been had he remained as a high school graduate. I assume an interest rate of 3.5% and a payment period of 40 years. I take the difference between the present value of the expected benefits and the actual costs. Costs equal the amount spent on tuition plus the number of years that students spend enrolled times the annual earnings for high school graduates. Previous estimates are a rough approximation of what the expected benefits of pursuing different alternatives may be for students. Earnings for students in my sample may be different to the average earnings for students who have graduated from a given degree, earnings may also evolve through the life cycle, and non-graduates may retrieve some benefit from their years enrolled in higher education. Still, previous estimates provide a picture of what we can expect students to earn given their choices and graduation rates, and how these numbers compare to the costs of attending higher education for these students.

Results indicate that giving students access to loans for universities increases the amount of time and money that students spend on higher education without substantially increasing their expected earnings. Figures 2.5 and Table 2.8 begin by looking at the impact that each program has on graduation rates. Figure 2.5 (b) shows that giving students access to loans for universities, above loans for technical schools, decreases the probability that students will graduate from a technical school while increasing the probability that they will graduate from a university by roughly the same amount. Results in Table 2.8 confirm the visual analysis, showing a decrease in the probability of graduating from a technical school of 7.1 p.p. and an increase in the probability of graduating from a university of 6 p.p.. There is no major change in the probability of graduating from any degree.

In terms of how much students spend, results in Table 2.9 show that 7 to 9 years after high

school graduation, students who had access to loans for universities above loans for technical schools have spent 0.18 additional years enrolled and 1,076 additional USD on tuition. This is partly a consequence of students choosing lengthier and more expensive higher education alternatives. Nevertheless, when looking at expected earnings for students, I observe that expected employment rates have increased by 0.5 p.p., and that expected annual earnings have increased by just 136 USD. Taken together these numbers imply a net present value of just 229 USD. Depending on the interest rate that is assumed for the present value estimates and how we assume that wages will evolve throughout time, this number may vary, taking positive or negative values. Still, even in the most optimistic scenario, net present values estimates are relatively low (see Appendix 2.C for a sensitivity analysis of present value estimates).

Low net present value estimates are not a consequence of students choosing low-return university alternatives. In fact, had students graduated from the degrees that they chose in the first place, expected incomes would have increased by 651 USD, which would more than justify the additional time and money spent on higher education. Instead, results indicate that students have a hard time succeeding at these more demanding alternatives and end-up dropping out or switching into lower quality alternatives. Previous results can also be seen in Figure 2.6 (b) which shows how the amount of time and money that students spend on higher education increases right at the discontinuity for program reception while showing no major increase in expected annual incomes at the discontinuity.

Results are even less encouraging for students who do not have access to loans and receive access to both loans for universities and technical schools. Figure 2.5 (c) shows that for these students gaining access to loans decreases their probability of graduating from a technical degree, without increasing their chances of graduating from a university degree. This leads to an overall decrease in the probability of graduating from any degree. Results in Table 2.8 confirm the visual analysis and show that for these students gaining access to loans decreases their probability of graduating from a technical degree in 3 p.p., and their overall probability

of graduating from any degree in 2 p.p. As shown in Table 2.9, seven to nine years after high school graduation, these students have spent 0.3 additional years enrolled and 960 additional USD on tuition, which is also partly a consequence of students choosing lengthier and more expensive alternatives. However, expected earnings and employment rates for these students remain roughly unchanged. Previous numbers imply a net present value for these students of minus 5,703 USD. The magnitude of this estimate varies depending on the interest rate and wage growth assumed. However, regardless of the parameters used, net present value estimates are always negative for this group (Appendix 2.C). In line with previous findings, results are not a consequence of students choosing low-return university alternatives, but rather of students being unable to graduate from more-demanding alternatives. Results can also be seen in Figure 2.6 (c) which, once again, shows how the amount of time and money that students spend in higher education increases right at the discontinuity for program reception while showing no increase in expected annual incomes at the discontinuity.

Results are somewhat better for students who do not have access to loans and gain access to loans for technical schools. Figure 2.5 (a) shows that giving these students access to loans for technical schools increases their probability of graduating from a professional degree at a technical school, without significantly decreasing their probability of graduating from a university degree, which leads to an increase in their overall graduation rates. Results in Table 2.8 show that, indeed, the probability of graduating from a professional degree at a technical school increases in 2 p.p. for these students, and that the overall probability of graduating from any degree increases in 1.6 p.p. Expected earnings and employment rates remain roughly unchanged for these students. However, while we should have expected a small increase in the number of years and money that students spend in higher education, in practice we observe that students spend 0.15 additional years enrolled and 371 additional USD on tuition. Suggesting that students persist longer before dropping out. Because of the latter, the net present value for these students decreases in 1,897 USD. Here too the magnitude of this estimate varies depending on the interest rate and wage growth assumed,

but is always negative (Appendix 2.C). This result can also be seen in Figure 2.6 (c) which shows how the amount of time and money that students spend increases at the discontinuity for program reception, while expected earnings remain roughly unchanged. These students are somewhat better compared to students who were given access to loans for universities and technical schools. However, net present values are still negative for these students.

E Heterogeneous effects

Previous results could vary by students' ability, which is why in this section I proceed to look at heterogeneous effects by students' GPA and test score performance. Although regression discontinuity estimates only allow me to look at local effects, I combine the two measures of student ability that I have to look at the impact of giving students access to loans for technical schools for students with varying test score performance; and to look at the effect of giving loans for universities, above loans for technical schools, for students with varying GPA (Appendix 2.D shows that baseline variables are balanced across the different sub-samples analyzed). I choose not to look at heterogeneous effects across the sub-sample in the margin of getting access to state loans for both universities and technical schools because there is little variation in students' GPA.

Table 2.10 looks at heterogeneous effects by test score performance for students who do not have access to loans and are on the margin of getting access to loans for technical schools. I divide students into three equivalent groups based on their test score performance to have as much statistical power as possible. Results show that loans for technical schools typically switch students away from universities and into professional degrees at technical schools. This is not the case, however, for very low-performing students who have little chances of enrolling in a university. For these students, loans for technical schools increase enrollment at professional degrees in technical schools without decreasing university degrees significantly. Looking at graduation rates I observe that for students who are in the middle in terms of academic performance, loans for technical schools help to increase graduation rates

from technical schools, without a major decrease in graduation rates from universities. This leads to an overall increase in graduation rates of 5.4 p.p. Meanwhile, for relatively higher-performing students the program increases graduation rates from technical schools while also decreasing graduation rates from universities, leading to a null effect on overall graduation rates. Also, while the time and money that students spend in higher education increase significantly for the three sub-samples analyzed, the increase in costs is somewhat smaller for students who are in the middle in terms of academic performance. Overall, previous results lead to the general finding that while loans for technical schools substantially reduce the net present value for relatively high-performing students, the effects are less negative for students who are in the middle in terms of academic performance.

Next, Table 2.11 looks at heterogeneous effects by students' GPA for students on the margin of getting access to loans for university degrees, above loans for technical schools. I divide students into three equivalent groups based on their GPA to have as much statistical power as possible. As can be seen in Table 2.11, it is always the case that loans for universities switch students away from technical schools and into universities, increasing university graduation rates, while decreasing technical schools graduation rates by roughly the same amount. However, while loans for university degrees do not increase expected income and employment for students with low-GPAs, they do increase expected earnings and employment for students with relatively high-GPAs. Overall, the increased benefits seem to outweigh the extra costs for high-GPA students, but not for low-GPA students.

VI Conclusion

This paper analyses and compares the long-term effects of two loan programs in Chile: one that provides students with loans for technical schools, and one that gives students access to loans for both technical schools and universities.

We can learn several important lessons from the results of this study. First, results show

that neither of these programs has a major effect on students' probability of ever enrolling in higher education. Suggesting that, despite being low-performing and having little access to financial aid, in the long-run credit constraints are not a major impediment for higher education access for students on the margin of getting access to loans in Chile.

Second, while not having a major effect on students' probability of ever enrolling in higher education, results show that loans do affect where students choose to go. Giving students loans for universities switches students away from technical schools and into universities. Instead, giving students access to loans exclusively for technical schools switches students away from universities and into professional degrees at technical schools. The latter result is worth highlighting, as it shows that, when given loans exclusively for technical schools, students choose to substitute professional degrees at universities for professional degrees at technical schools, without increasing their demand for technical degrees. This is worth emphasizing given recent concerns about the potential shortage of technical workers in Chile.

Third, while loans apparently help students' to devote more time to schooling, there is no evidence that this leads to higher graduation rates. The high levels of dropout rates, reaching nearly 50% of higher education enrollees in Chile, is both a problem for students with or without loan access. This suggests that increasing financial aid is not the solution, or at least not the only solution, to increase higher education persistence rates.

Fourth, loans leave students in many cases worse-off, having increased the total amount of time and money that they spend in higher education without increasing their graduation rates or expected earnings. Results are particularly worrisome for low-performing students who, in response to loans, switch into more-demanding university alternatives where they have little chances of succeeding. Previous results point to the importance of match quality, as opposed to absolute quality, and highlight the potential costs of having low-performing students switch into more-demanding alternatives. This result is particularly relevant given recent policies in Chile offering free university education for low-income students regardless of their ability.

Fifth and last, while offering low-performing students loans for technical schools, as opposed to loans for universities seems to be a better alternative, this option is not without problems. Students who obtain loans for technical degrees increase significantly the time and money that they spend in higher education, which is not a result of them choosing more expensive alternatives, but instead apparently of students persisting longer before they drop out. Because of the latter, higher education costs increase for these students as well, without major increases in their expected earnings.

VII Figures

Figure 2.1: Program Design

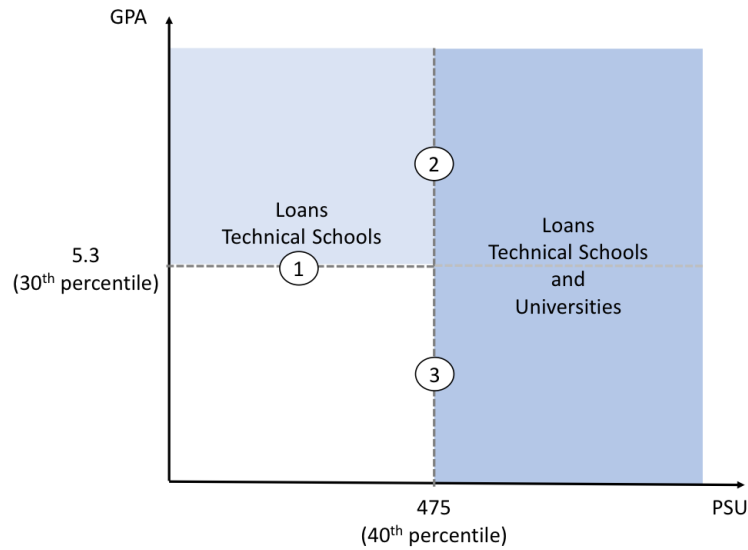


Figure 2.1 shows how loans for technical schools and universities are assigned in Chile, as well as how a RDD can be implemented to look at the causal impact of: (1) having access to state loans for technical schools for students who do not have access to state loans, (2) having access to state loans for universities for students who have access to state loans for technical schools, and (3) having access to state loans for both technical schools and universities for students who do not have access to state loans.

Figure 2.2: Distribution of Individuals

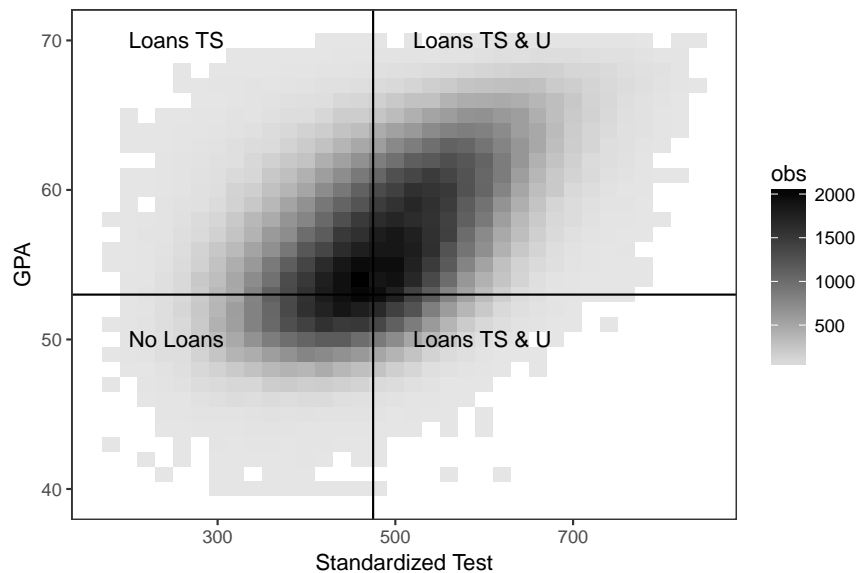
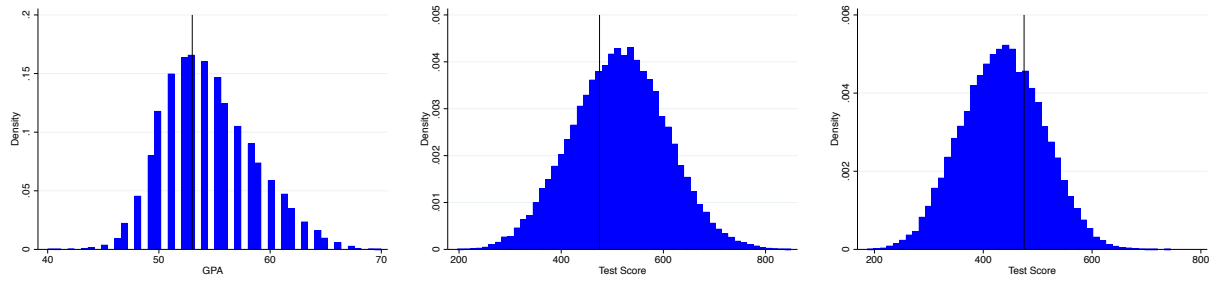


Figure 2.2 shows that there is sufficient density of observations at and across each of the three treatment thresholds I study in this paper.

Figure 2.3: Histograms

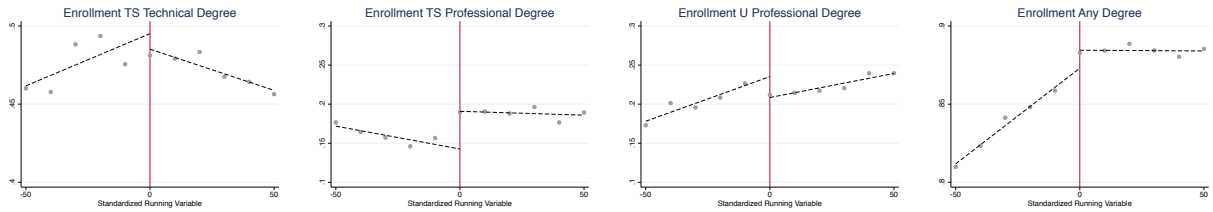


(a) Loans TS vs No Loans (b) Loans TS & U vs Loans TS (c) Loans TS & U vs No Loans

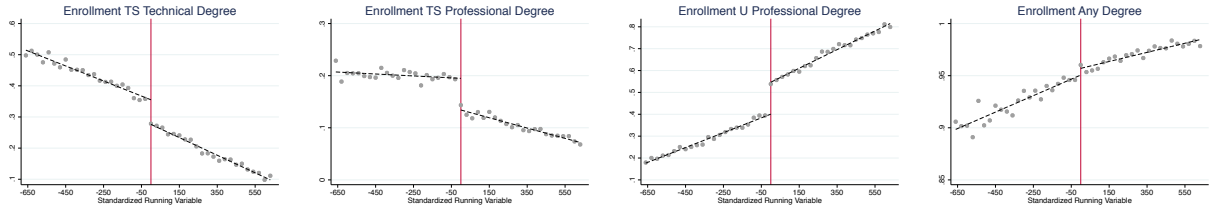
Figure 2.3 shows histograms of scores relative the three treatment thresholds I study in this paper.

Figure 2.4: Effect of Loan Access on Higher Education Enrollment

(a) A. Loans Technical Schools vs No Loans



(b) B. Loans Technical Schools & Universities vs Loans Technical Schools



(c) C. Loans Technical Schools & Universities vs No Loans

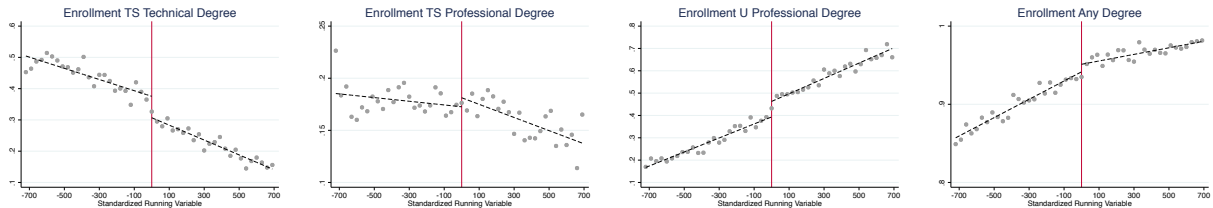
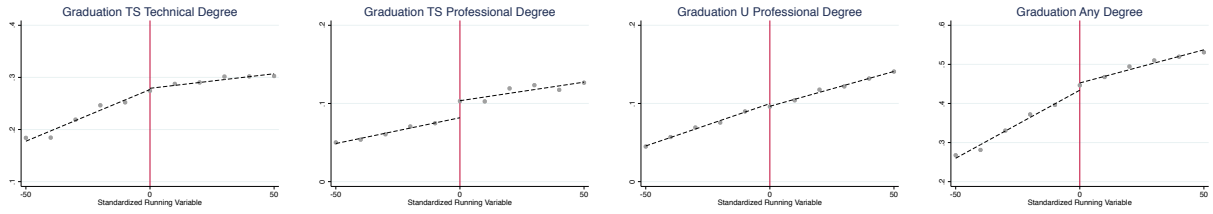


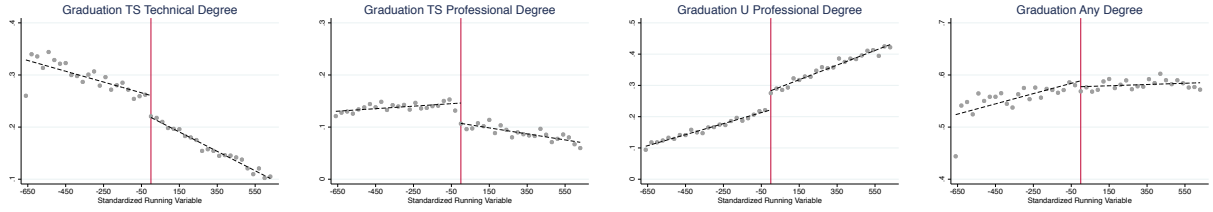
Figure 2.4 shows RD results for the probability of enrolling for the first time in a technical degree, a professional degree at a technical school, a professional degree at a university, or any degree. Grey dots present the average in the outcome variable for individuals in equally spaced disjoint bins. Black lines depict a linear fit for control and treatment units separately.

Figure 2.5: Effect of Loan Access on Higher Education Graduation

(a) A. Loans Technical Schools vs No Loans



(b) B. Loans Technical Schools & Universities vs Loans Technical Schools



(c) C. Loans Technical Schools & Universities vs No Loans

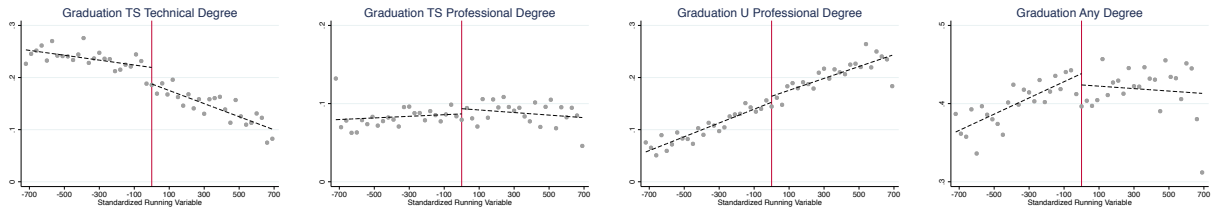
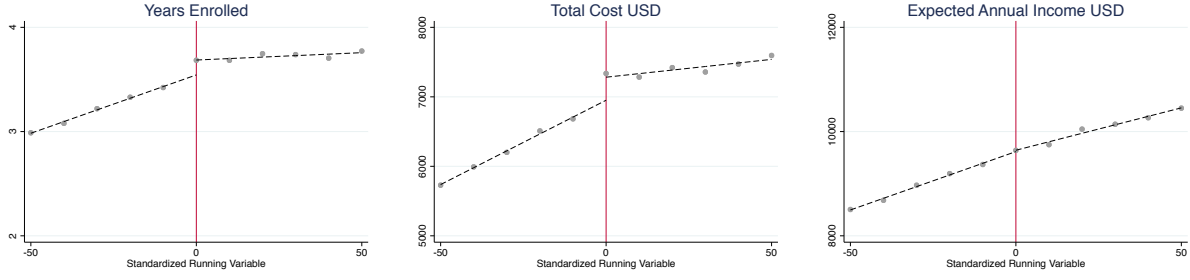


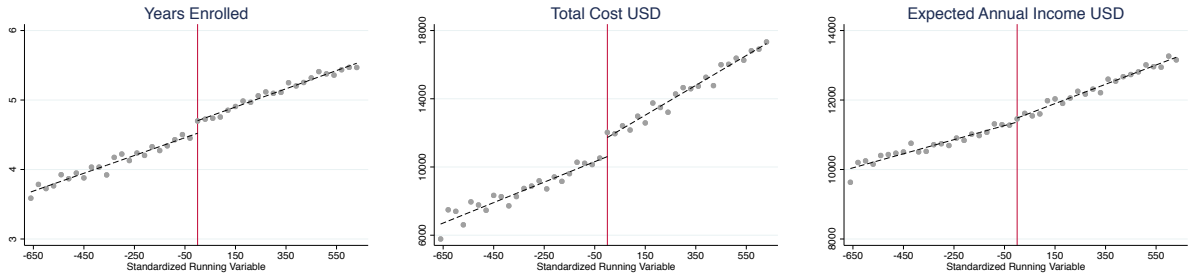
Figure 2.5 shows RD results for the probability of graduating from a technical degree, a professional degree at a technical school, a professional degree at a university, or any degree. Grey dots present the average in the outcome variable for individuals in equally spaced disjoint bins. Black lines depict a linear fit for control and treatment units separately.

Figure 2.6: Effect of Loan Access on Total Costs and Expected Income

(a) A. Loans Technical Schools vs No Loans



(b) B. Loans Technical Schools & Universities vs Loans Technical Schools



(c) C. Loans Technical Schools & Universities vs No Loans

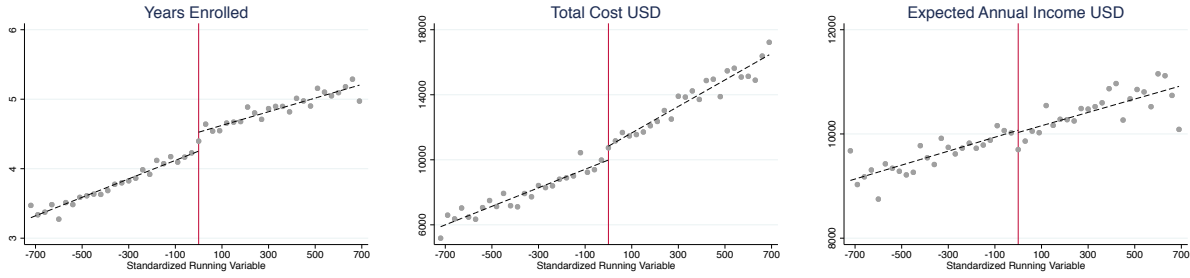


Figure 2.6 shows RD results for students' costs and expected benefits. Years and Total Cost refer to the amount of time and tuition money that students have spent 7 to 9 years after high school graduation. Expected Annual Income refers to students' expected earnings four years after graduation based on where they have graduated from. I assume an annual wage of 8,844 USD and an employment rate of 0.55 if students do not graduate or never enroll in higher education (see Section D for details). Grey dots present the average in the outcome variable for individuals in equally spaced disjoint bins. Black lines depict a linear fit for control and treatment units separately.

VIII Tables

Table 2.1: Characteristics of the Degrees Offered by Technical Schools and Universities

	Technical Schools		Universities
	Technical Degrees	Professional Degrees	
Enrollment	0.19	0.13	0.68
Degree Length	2.53	4.22	4.05
	(0.52)	(0.55)	(1.68)
Degree Annual Cost (2017 USD)	1,821	2,365	3,923
	(603)	(686)	(2,177)
Degree Total Cost (2017 USD)	3,867	9,209	14,982
	(1,418)	(3,055)	(9,206)
Degree Percentile Selectivity	37	43	74
	(13)	(13)	(20)
Degree Expected Employment	0.66	0.71	0.78
	(0.13)	(0.12)	(0.10)
Expected Annual Earnings (2017 USD)	12,898	15,152	20,735
	(3,961)	(3,658)	(7,049)

Table 2.1 presents descriptive characteristics for degrees offered by technical schools and universities in 2008. Standard deviations are reported in parenthesis. Enrollment equals the enrollment captured by each type of institution in 2008. Degree Length equals number of years it takes to complete the degree on-time. Degree Annual and Total Costs equal annual and total tuition costs. Degree Expected Employment equals expected employment rates one year after graduation. Degree Expected Annual Earnings equals expected annual income four years after graduation.

Table 2.2: Sample

	All		Loans TS vs No Loans		Loans TS & U vs Loans TS		Loans TS & U vs No Loans	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Socioeconomic Characteristics								
Female	0.53	(0.50)	0.60	(0.49)	0.62	(0.49)	0.46	(0.50)
Age	18.37	(3.05)	18.27	(2.41)	17.92	(1.90)	18.21	(1.86)
Lives in the capital	0.40	(0.49)	0.30	(0.46)	0.30	(0.46)	0.41	(0.49)
Public School	0.43	(0.49)	0.53	(0.50)	0.49	(0.50)	0.38	(0.49)
Private Voucher School	0.48	(0.50)	0.46	(0.50)	0.50	(0.50)	0.59	(0.49)
Private School	0.10	(0.30)	0.01	(0.09)	0.01	(0.10)	0.02	(0.15)
Total HH members	4.45	(1.89)	4.54	(1.81)	4.49	(1.68)	4.51	(1.76)
Total HH members work	1.22	(0.76)	1.18	(0.72)	1.14	(0.67)	1.21	(0.72)
Head of HH father	0.62	(0.49)	0.59	(0.49)	0.61	(0.49)	0.57	(0.50)
Head of HH mother	0.28	(0.45)	0.29	(0.45)	0.29	(0.45)	0.33	(0.47)
Annual Income (2017 USD)	8,859	(8,917)	5,185	(4,076)	5,914	(4,609)	6,549	(4,907)
Mother primary ed	0.24	(0.43)	0.30	(0.46)	0.25	(0.43)	0.18	(0.38)
Mother secondary ed	0.52	(0.50)	0.58	(0.49)	0.59	(0.49)	0.62	(0.49)
Mother tertiary ed	0.25	(0.43)	0.12	(0.32)	0.16	(0.37)	0.21	(0.40)
Father primary ed	0.23	(0.42)	0.30	(0.46)	0.25	(0.43)	0.17	(0.38)
Father secondary ed	0.47	(0.50)	0.54	(0.50)	0.54	(0.50)	0.57	(0.49)
Father tertiary ed	0.30	(0.46)	0.17	(0.37)	0.21	(0.40)	0.26	(0.44)
Father works full-time	0.63	(0.48)	0.57	(0.50)	0.58	(0.49)	0.61	(0.49)
Mother works full-time	0.33	(0.47)	0.28	(0.45)	0.29	(0.45)	0.35	(0.48)
Academic Performance								
GPA	5.64	(0.57)	5.32	(0.27)	5.75	(0.34)	5.15	(0.44)
Language Score	486	(111)	405	(64)	480	(49)	466	(52)
Math Score	489	(113)	405	(65)	483	(50)	466	(51)
Enrollment								
Enrolls Tech. Inst.	0.36	(0.48)	0.65	(0.48)	0.43	(0.50)	0.53	(0.50)
Enrolls Univ.	0.46	(0.50)	0.22	(0.41)	0.52	(0.50)	0.39	(0.49)
Enrolls Any	0.82	(0.38)	0.87	(0.34)	0.95	(0.21)	0.92	(0.26)
Graduation								
Graduates Tech. Inst.	0.19	(0.39)	0.34	(0.47)	0.31	(0.46)	0.27	(0.45)
Graduates Univ.	0.23	(0.42)	0.10	(0.30)	0.28	(0.45)	0.14	(0.35)
Graduates Any	0.42	(0.49)	0.43	(0.50)	0.57	(0.49)	0.41	(0.49)
Obs	584,759		96,719		100,788		35,543	

Table 2.2 compares descriptive characteristics for students who completed high school between 2007 and 2009 and who signed up for the standardized college admission on the year they graduated from high school, to those of students in the three sub-samples analyzed.

Table 2.3: Balance

	Loans TS vs No Loans		Loans TS & U vs Loans TS		Loans TS & U vs No Loans	
	Mean	T-C	Mean	T-C	Mean	T-C
	Control		Control		Control	
Female	0.611	-0.004 (0.006)	0.621	0.001 (0.006)	0.450	-0.010 (0.010)
Age	18.131	0.042 (0.028)	17.866	0.038 (0.024)	18.120	0.040 (0.036)
Lives in the capital	0.288	0.008 (0.006)	0.294	-0.004 (0.006)	0.422	-0.014 (0.010)
Public School	0.518	0.007 (0.006)	0.506	0.002 (0.006)	0.372	-0.017* (0.010)
Private Voucher School	0.477	-0.010* (0.006)	0.485	0.000 (0.006)	0.605	0.018* (0.010)
Private School	0.005	0.003*** (0.001)	0.008	-0.002** (0.001)	0.023	-0.001 (0.003)
Total HH members	4.515	0.015 (0.023)	4.519	-0.013 (0.021)	4.510	0.005 (0.036)
Total HH members work	1.163	0.012 (0.009)	1.139	0.004 (0.008)	1.210	-0.001 (0.015)
Total HH members in tertiary education	0.235	0.001 (0.006)	0.270	-0.005 (0.006)	0.321	-0.016 (0.011)
Head of HH father	0.583	0.007 (0.006)	0.612	-0.006 (0.006)	0.579	-0.015 (0.011)
Head of HH mother	0.301	-0.008 (0.006)	0.283	0.010* (0.006)	0.323	0.007 (0.010)
Annual Income (2017 USD)	5,215	53 (54)	5,648	-29 (55)	6,723	-164 (102)
Mother primary ed	0.287	0.008 (0.006)	0.262	-0.003 (0.006)	0.145	0.020** (0.008)
Mother secondary ed	0.591	-0.006 (0.007)	0.595	-0.001 (0.006)	0.644	-0.016 (0.010)
Mother tertiary ed	0.121	-0.002 (0.004)	0.143	0.004 (0.005)	0.211	-0.004 (0.009)
Father primary ed	0.280	0.010* (0.006)	0.263	-0.003 (0.006)	0.165	-0.012 (0.008)
Father secondary ed	0.545	-0.002 (0.007)	0.551	-0.005 (0.007)	0.572	0.010 (0.011)
Father tertiary ed	0.175	-0.008 (0.005)	0.185	0.007 (0.005)	0.263	0.002 (0.010)
Father works full-time	0.571	0.006 (0.007)	0.571	0.006 (0.007)	0.612	-0.001 (0.011)
Father works part-time	0.167	0.002 (0.005)	0.179	-0.002 (0.005)	0.131	-0.002 (0.007)
Mother works full-time	0.276	0.006 (0.006)	0.285	0.006 (0.006)	0.350	0.005 (0.010)
Mother works part-time	0.082	0.003 (0.004)	0.077	0.004 (0.003)	0.074	0.004 (0.006)
Obs	96,719		100,788		35,543	

Table 2.3 examines whether individuals just above and just below each of the three treatment thresholds are similar in terms of their observable characteristics. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2.4: Effect of Loan Access on Higher Education Enrollment

	Loans TS vs No Loans		Loans TS & U vs Loans TS		Loans TS & U vs No Loans	
	Mean Control	Est	Mean Control	Est	Mean Control	Est
Technical Degree						
Non-Accredited	0.098	−0.025*** (0.003)	0.040	−0.005** (0.002)	0.056	−0.020*** (0.005)
Accredited	0.393	0.017*** (0.006)	0.316	−0.070*** (0.005)	0.318	−0.045*** (0.009)
All	0.491	−0.008 (0.006)	0.356	−0.075*** (0.005)	0.374	−0.065*** (0.010)
Professional Degree Technical School						
Non-Accredited	0.015	−0.004*** (0.001)	0.007	−0.001 (0.001)	0.009	−0.001 (0.002)
Accredited	0.131	0.049*** (0.005)	0.190	−0.060*** (0.004)	0.167	0.010 (0.008)
All	0.146	0.045*** (0.005)	0.197	−0.061*** (0.004)	0.176	0.009 (0.008)
Professional Degree University						
Non-Accredited	0.046	−0.009*** (0.003)	0.033	−0.009*** (0.002)	0.045	−0.011*** (0.004)
Accredited	0.189	−0.014*** (0.005)	0.364	0.152*** (0.006)	0.344	0.079*** (0.009)
All	0.235	−0.023*** (0.005)	0.397	0.142*** (0.006)	0.389	0.068*** (0.009)
Any Degree						
All	0.872	0.014*** (0.004)	0.950	0.006** (0.003)	0.940	0.012** (0.005)
Obs	96,719		100,788		35,543	

Table 2.4 shows RD estimates of the impact that each treatment has on students' probability of enrolling for the first time in an accredited/non-accredited: technical degree, professional degree at a technical school, professional degree at a university, or any degree. Variables equal zero if an individual never enrolls in higher education. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2.5: Examples of degrees chosen by students in the control group

Sample	Type of Degree	Examples degrees
Loans TS vs No Loans	Technical Degrees: Professional Degrees TS: Professional Degrees U:	nursery, risk prevention, gastronomy, mechanic risk prevention, psychopedagogy, social work, nursery education physical education, social work, nursery, primary education
Loans TS & U vs Loans TS	Technical Degrees: Professional Degrees TS: Professional Degrees U:	nursery, business administration, risk prevention, gastronomy risk prevention, social work, psychopedagogy, accountant nursery, physical education, kinesiology, primary education
Loans TS & U vs No Loans	Technical Degrees: Professional Degrees TS: Professional Degrees U:	nursery, risk prevention, gastronomy, business administration risk prevention, psychopedagogy, graphic design, social work physical education, social work, nursery, kinesiology

Table 2.5 shows examples of the most common technical degrees, professional degrees at technical schools and professional degrees at universities chosen by students in each of the three control groups.

Table 2.6: Effect of Loan Access on the Characteristics of the Chosen Degrees

	Loans TS vs No Loans		Loans TS & U vs Loans TS		Loans TS & U vs No Loans	
	Mean Control	Est	Mean Control	Est	Mean Control	Est
Degree Characteristics						
Length	2.901	0.079*** (0.019)	3.597	0.212*** (0.017)	3.491	0.219*** (0.029)
Annual Cost	2,218	51*** (16)	2,867	239*** (17)	2,812	216*** (29)
Total Cost	7,390	183** (79)	10,918	1,478*** (98)	10,503	1,248*** (159)
Selectivity	37.253	0.708*** (0.255)	50.020	4.581*** (0.255)	47.843	2.848*** (0.407)
Degree Field						
Non-STEM	0.294	0.003 (0.006)	0.310	0.025*** (0.006)	0.339	0.028*** (0.010)
STEM	0.248	0.003 (0.005)	0.270	0.001 (0.005)	0.299	−0.014 (0.009)
Business & Adm	0.133	0.002 (0.004)	0.162	−0.008* (0.004)	0.146	−0.006 (0.007)
Health	0.197	0.006 (0.005)	0.207	−0.012** (0.005)	0.155	0.004 (0.007)
Degree Expected Outcomes						
Expected Graduation	0.517	0.018*** (0.003)	0.595	0.007*** (0.002)	0.564	0.010** (0.004)
Expected Employment	0.649	−0.002 (0.002)	0.685	0.013*** (0.002)	0.684	0.001 (0.003)
Expected Annual Earnings (2017 USD)	13,105	−43 (58)	14,951	651*** (69)	14,700	349*** (105)
Obs	96,719		100,788		35,543	

Table 2.6 shows RD estimates of the impact that each treatment has on the characteristics of the degrees where students enroll for the first time. Length equals number of years it takes to complete the degree on-time. Annual and Total Costs equal annual and total tuition costs. Expected Graduation equals the probability that a student who entered that degree will graduate 10 years later. These variables all equal zero if an individual never enrolls in higher education. Expected Employment equals expected employment rates one year after graduation. Expected Annual Earnings equals expected annual income four years after graduation. I assume an annual wage of 8,844 USD and an employment rate of 0.55 if students never enroll in higher education. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2.7: Effect of Loans Access on Financial Aid and Time to Schooling

	Loans TS vs No Loans		Loans TS & U vs Loans TS		Loans TS & U vs No Loans	
	Mean	Est	Mean	Est	Mean	Est
	Control		Control		Control	
Financial Aid in the First Year (2017 USD)						
Annual Cost	2,218	51*** (16)	2,867	239*** (17)	2,812	216*** (29)
Grants Received	402	-88*** (5)	290	1 (7)	173	-29*** (7)
Credit Access	230	983*** (13)	1,552	798*** (17)	536	1,808*** (25)
Annual Cost- (Credit+Grants)	1,573	-861*** (16)	992	-547*** (13)	2,078	-1,568*** (23)
Time to schooling						
Full-time Student	0.668	0.039*** (0.006)	0.809	0.025*** (0.005)	0.766	0.045*** (0.008)
Enrolls on-time	0.495	0.057*** (0.006)	0.565	0.094*** (0.006)	0.534	0.134*** (0.010)
N of Degrees Enrolled	1.425	0.011 (0.012)	1.525	-0.012 (0.010)	1.620	0.030 (0.019)
Gap Years	0.522	-0.043*** (0.013)	0.435	-0.005 (0.011)	0.566	-0.025 (0.022)
Obs	96,719		100,788		35,543	

Table 2.7 shows RD estimates of the impact that each treatment has on students' higher education costs, financial aid, and time devoted to schooling. Financial Aid in the First Year measures the tuition costs, grants and credit access that students face the first year they enroll in higher education. Time to schooling measures a students' probability of enrolling full-time, his probability of enrolling on-time (i.e. immediately after high school graduation), number of degrees where he enrolls, and number of gap years that he takes while enrolled. All variables equal zero if an individual never enrolls in higher education (See Section C for details) *** p < 0.01, ** p < 0.05, * p < 0.10.

Table 2.8: Effect of Loan Access on Graduation

	Loans TS vs No Loans		Loans TS & U vs Loans TS		Loans TS & U vs No Loans	
	Mean	Est	Mean	Est	Mean	Est
	Control		Control		Control	
Technical Degree	0.278	0.000 (0.006)	0.255	-0.034*** (0.005)	0.218	-0.030*** (0.008)
Professional Degree TS	0.082	0.020*** (0.004)	0.145	-0.037*** (0.004)	0.091	-0.000 (0.006)
Professional Degree Univ	0.100	-0.003 (0.004)	0.219	0.060*** (0.005)	0.152	0.007 (0.007)
Any Degree	0.436	0.016** (0.006)	0.580	-0.004 (0.006)	0.439	-0.022** (0.010)
Obs	96,719		100,788		35,543	

Table 2.8 shows RD estimates of the impact that each treatment has on students' probability of graduating from: a technical degree, a professional degree at a technical school, a professional degree at a university, or any degree. Variables equal zero if an individual never enrolls or never graduates from higher education. *** p < 0.01, ** p < 0.05, * p < 0.10.

Table 2.9: Effect of Loan Access on Costs and Expected Benefits

	Loans TS vs No Loans		Loans TS & U vs Loans TS		Loans TS & U vs No Loans	
	Mean	Est	Mean	Est	Mean	Est
	Control		Control		Control	
Costs						
<i>Expected Costs (Given First Enrollment)</i>						
Total Years	2.901	0.079*** (0.019)	3.597	0.212*** (0.017)	3.491	0.219*** (0.029)
Total Cost (2017 USD)	7,390	183** (79)	10,918	1,478*** (98)	10,503	1,248*** (159)
<i>Actual Costs (At Graduation)</i>						
Total Years	3.542	0.150*** (0.027)	4.498	0.179*** (0.024)	4.247	0.261*** (0.042)
Total Cost (2017 USD)	6,946	371*** (86)	10,495	1,076*** (114)	9,857	960*** (170)
Benefits						
<i>Expected Benefits (Given First Enrollment)</i>						
Expected Annual Earnings (2017 USD)	13,105	-43 (58)	14,951	651*** (69)	14,700	349*** (105)
Expected Employment	0.649	-0.002 (0.002)	0.685	0.013*** (0.002)	0.684	0.001 (0.003)
<i>Expected Benefits (At Graduation)</i>						
Expected Annual Earnings (2017 USD)	10,624	-9 (45)	12,055	136** (62)	11,098	-113 (80)
Expected Employment	0.592	-0.000 (0.001)	0.628	0.005*** (0.001)	0.607	-0.005** (0.002)
Benefits-Costs						
PV Benefits-Costs	-208	-1,956** (923)	18,271	229 (1,302)	738	-5,703*** (1,683)
Obs	96,719		100,788		35,543	

Table 2.9 shows RD estimates of the impact that each treatment has on students' costs and expected benefits. Expected Costs (Given First Enrollment) refer to the expected amount of time and tuition money that students would have spent if they had graduated on-time from the degree that they chose in the first place. Actual Costs (At Graduation) refer to how much they have actually spent 7 to 9 years after high school graduation. Expected Benefits (Given First Enrollment) refer to the expected annual income that students would earn four years after graduation and their employment probability one year after graduation if they had graduated from the degree that they chose in the first place. Expected Benefits (At Graduation) refer to the expected earnings and employment rates based on where they have actually graduated from. I assume an expected annual wage of 8,844 USD and an expected employment rate of 0.55 for students who do not graduate or never enroll in higher education. PV Benefits-Costs are present-discounted expected earnings net of costs, assuming an interest rate of 3.5% and zero wage growth (see Section D for details). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2.10: Effect of Loans TS vs No Loans for Students of Varying Test Score Performance

	≤ 381		381-433		> 433	
	Mean Control	Est	Mean Control	Est	Mean Control	Est
Enrollment						
Technical Degree	0.547	-0.011 (0.011)	0.499	0.009 (0.011)	0.430	-0.020* (0.011)
Professional Degree TS	0.121	0.037*** (0.008)	0.160	0.040*** (0.009)	0.153	0.057*** (0.009)
Professional Degree Univ	0.125	-0.008 (0.007)	0.222	-0.028*** (0.009)	0.352	-0.031*** (0.010)
Any Degree	0.793	0.018** (0.009)	0.880	0.021*** (0.007)	0.935	0.007 (0.006)
Graduation						
Technical Degree	0.275	-0.005 (0.009)	0.278	0.027*** (0.010)	0.276	-0.016 (0.010)
Professional Degree TS	0.058	0.013** (0.005)	0.085	0.024*** (0.007)	0.104	0.023*** (0.007)
Professional Degree Univ	0.045	0.000 (0.004)	0.089	0.010 (0.006)	0.165	-0.018** (0.008)
Any Degree	0.361	0.009 (0.010)	0.426	0.054*** (0.011)	0.514	-0.011 (0.011)
Actual Costs (At Graduation)						
Total Years	2.822	0.158*** (0.045)	3.579	0.169*** (0.046)	4.167	0.142*** (0.046)
Total Cost (2017 USD)	4,613	480*** (113)	6,914	242* (142)	9,165	416** (178)
Expected Benefits (At Graduation)						
Expected Annual Earnings (2017 USD)	9,961	16 (63)	10,589	54 (77)	11,303	-88 (91)
Expected Employment	0.574	0.000 (0.002)	0.591	0.003 (0.002)	0.612	-0.004* (0.002)
Benefits-Costs						
PV Benefits-Costs	-5,636	-1,548 (1,291)	-1,257	-661 (1,579)	6,560	-3,674* (1,899)
Obs	32,142		31,733		32,844	

Table 2.10 shows RD estimates of the impact of having access to loans for technical schools for students of varying test score performance. Actual Costs (At Graduation) refer to how much students have spent 7 to 9 years after high school graduation. Expected Benefits (At Graduation) refer to expected annual incomes four years after graduation and employment probabilities one year after graduation based on where students have graduated from. I assume an expected annual wage of 8,844 USD and an expected employment rate of 0.55 for students who do not graduate or never enroll in higher education. PV Benefits-Costs are present-discounted expected earnings net of costs, assuming an interest rate of 3.5% and zero wage growth (see Section D for details). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2.11: Effect of Loans TS & U vs Loans TS for Students of Varying GPA

	≤ 5.5		5.5-5.8		> 5.8	
	Mean Control	Est	Mean Control	Est	Mean Control	Est
Enrollment						
Technical Degree	0.361	-0.064*** (0.009)	0.356	-0.083*** (0.010)	0.352	-0.080*** (0.009)
Professional Degree TS	0.212	-0.050*** (0.008)	0.188	-0.055*** (0.008)	0.186	-0.077*** (0.007)
Professional Degree Univ	0.380	0.116*** (0.009)	0.406	0.149*** (0.010)	0.411	0.163*** (0.009)
Any Degree	0.952	0.002 (0.004)	0.949	0.011** (0.005)	0.949	0.005 (0.004)
Graduation						
Technical Degree	0.235	-0.023*** (0.008)	0.252	-0.035*** (0.009)	0.283	-0.051*** (0.009)
Professional Degree TS	0.137	-0.029*** (0.007)	0.148	-0.042*** (0.007)	0.149	-0.044*** (0.007)
Professional Degree Univ	0.189	0.037*** (0.008)	0.222	0.062*** (0.010)	0.251	0.083*** (0.010)
Any Degree	0.529	-0.010 (0.010)	0.578	-0.001 (0.011)	0.642	-0.005 (0.010)
Actual Costs (At Graduation)						
Total Years	4.525	0.094** (0.040)	4.525	0.229*** (0.043)	4.445	0.219*** (0.041)
Total Cost (2017 USD)	10,600	792*** (184)	10,613	1,289*** (209)	10,270	1,156*** (205)
Expected Benefits (At Graduation)						
Expected Annual Earnings (2017 USD)	11,681	33 (92)	12,073	69 (110)	12,467	310*** (117)
Expected Employment	0.619	0.002 (0.002)	0.627	0.005* (0.003)	0.638	0.008*** (0.003)
Benefits-Costs						
PV Benefits-Costs	9,918	-894 (1,939)	18,310	-1,855 (2,321)	27,772	3,476 (2,475)
Obs	35,159		30,051		35,577	

Table 2.11 shows RD estimates of the impact that loans for universities above loans for technical schools have on students of varying GPA. Actual Costs (At Graduation) refer to how much students have spent 7 to 9 years after high school graduation. Expected Benefits (At Graduation) refer to expected annual incomes four years after graduation and employment probabilities one year after graduation based on where students have graduated from. I assume an expected annual wage of 8,844 USD and an expected employment rate of 0.55 for students who do not graduate or never enroll in higher education. PV Benefits-Costs are present-discounted expected earnings net of costs, assuming an interest rate of 3.5% and zero wage growth (see Section D for details). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Chapter 3

Walking in Your Footsteps: Sibling Spillovers in Higher
Education Choices

I Introduction

Few decisions in life are as complex and determinant of a person’s future as choosing a college and a major. Only in the state of California, a high school graduate can choose among 435 higher education institutions, each of which conducts to a diverse array of specializations. This diversity is typical of higher education systems which, like the U.S., experienced periods of unprecedented growth in college enrollment. Such is the case of Chile, where total college enrollment increased by a factor of 3.2 between 1997 and 2017, and where the number of institutions as a proportion of the population is almost the same as in California.¹ If, to the large number of alternatives, we add the considerable heterogeneity in the economic returns to different colleges and fields of study documented in recent empirical work ([Kirkeboen et al., 2016](#); [Hastings et al., 2013](#)), we are left with the definition of an economic problem: students with limited information, facing a myriad of different possibilities that might either limit or enable their lifetime economic potential. Given the complexity of this choice, the abundance of academic literature dedicated to studying the determinants of higher education choices should come as no surprise.²

More surprising is the fact that, despite the prominence of social interactions in contemporary educational research, very little attention has been paid to their role in shaping students’ higher education choices. Social networks can provide students with precious and otherwise costly information about the costs and benefits of attending a particular institution or choosing a given field of study.³ Once in college, social networks can be a valuable source

¹Approximately 1 institution per 100,000 inhabitants in Chile, versus 1.1 in California.

²This literature has studied a number of determinants including financial aid ([Avery and Hoxby, 2004](#); [Dynarski, 2003b](#); [Hurwitz, 2012](#)), distance ([Leppel, 1993](#); [DesJardins et al., 1999](#)), and college quality ([Long, 2004](#); [Luca and Smith, 2013](#)). Some less obvious determinants have also been looked at, such as the effects of raising application fees ([Pallais, 2015b](#); [Smith et al., 2015b](#)), the effects of having the university sport team succeed ([Pope and Pope, 2009](#)), the effects of granting small amounts of merit aid ([Cohodes and Goodman, 2014](#)), and the effects of assisting students in their application to financial aid ([Bettinger et al., 2012b](#)).

³There is a broad literature indicating that individuals are unaware about the returns of investing in education in general, as well as the returns to investing in specific majors, and that information interventions can be important in determining their choices. See [Wiswall and Zafar \(2014\)](#); [Zafar \(2011\)](#); [Arcidiacono et al. \(2012\)](#); [Stinebrickner and Stinebrickner \(2013\)](#); [Jensen \(2010\)](#); [Nguyen \(2008\)](#); [Oreopoulos and Dunn \(2013\)](#); [Dinkelman and Martínez \(2014\)](#)

of academic and emotional support, thus increasing the value for a student of attending college together with their friends, peers or family members. In addition, attending college together with other members in a student’s social network may result in cost reductions in items such as housing or commuting. Finally, students may have a tendency to imitate the choices of people they admire in their social networks because they assign a value to being perceived (or perceive themselves) as similar to them ([Akerlof and Kranton, 2002](#)). Although these factors have been mentioned in the literature as potentially relevant, very few papers have pursued systematic efforts to identify causal links between higher education choices of students who are socially connected. This paper contributes to closing this gap by presenting credible empirical evidence of a causal relationship between higher education choices of older and younger siblings.

In part lack of evidence on the effects of social network on higher education choices reflects an identification challenge. Several papers have noted the difficulties associated with analyzing peer effects in general, which include the so-called *reflection problem* and the endogeneity problem ([Manski, 1993](#); [Moffitt et al., 2001](#)). In this paper, we are able to overcome the *reflection problem* by focusing on the influence of older siblings’ enrollment on the subsequent choices of younger siblings; and the endogeneity problem by exploiting discontinuous admission rules generated by Chile’s centralized system of admission to higher education. In Chile, like most other countries, but unlike the U.S., students need to choose both a major and an institution at the moment of applying. The system is such that an applicant is offered admission to her most preferred major/institution combination for which her score is above a program-specific cutoff determined by the number of available seats. These discontinuities provide an exogenous source of variation for the enrollment decisions of older siblings that is arguably unrelated to any factors simultaneously affecting the choices of older and younger siblings. Our research design is thus based on the idea that differences between the choices of younger siblings of students who were marginally admitted to a given major/institution and the choices of younger siblings of students who

were marginally rejected from the same major/institution can be reasonably attributed to the influence of the older sibling's enrollment in that program. The same strategy has been used by [Kirkeboen et al. \(2016\)](#) and [Hastings et al. \(2013\)](#) to look at the returns by field of study.

Our empirical findings reveal strikingly large sibling spillovers in higher education choices. Having an older sibling enrolling, as opposed to just applying, to a given major/institution increases by 1.3 p.p. the likelihood of enrolling in the same major/institution, representing an 87% increase. Spillover effects do not restrict to just major/institution combinations, but also to other majors within the same college. Having an older sibling enroll in a given major/institution increases the younger siblings' probability of choosing any major within the same college in 4.2 p.p., representing a 25% increase. In contrast, we find no evidence of an increase in the younger sibling's probability of attending the same major in a different college.

Previous estimates are all the more surprising considering that we are comparing individuals whose older siblings have both applied to a given major/institution, and are in the margin of being admitted. Moreover, because of how our empirical strategy is designed, our counterfactual must be thought of as having an older sibling enroll either: in a different major within the same institution, the same major in a different institution, or a different major in a different institution. In fact, exploiting information on older siblings' next-best alternatives, we can show how our estimates vary when we alter the counterfactual. For instance, having an older sibling enroll in a major/institution has no effect on the younger sibling's probability of choosing any major within that college when the counterfactual is a different major within that same institution. However, it can increase the younger sibling's probability of choosing any major within that college by as much as 6.4 p.p. when the counterfactual is a different institution, representing a 51% increase.

To get an idea of what these magnitudes represent, we can compare them to other effects identified in the literature on the determinants of college choice. For instance, [Avery and](#)

Hoxby (2004) estimate a multinomial logit model of college choice and find that a US \$1,000 college-specific grant increases the probability of choosing a college by 10.8%. Assuming linearity, and ignoring contextual differences, having an older sibling enroll in a college would be equivalent to offering the student a \$4,722 grant to enroll in that college.⁴

An analysis of heterogeneous effects allows us to get a better understanding of the mechanisms that could be driving these effects. We find no evidence that spillover effects across major/institution combinations are driven by increased information. Results remain stable across students who have had more or less high school peers attending the institution in question, and who are more or less likely to be informed about a program. Suggesting that effects are not driven by increased information, or at least not by information that can be easily gathered from other sources or social interactions. Moreover, results also remain stable regardless of whether the program in question is expected to be a good or bad match for the younger siblings (where match is defined based on the younger sibling's probability of actually graduating from that program). Suggesting that younger siblings are willing to follow their older siblings even towards programs where they have little chances of succeeding; and discarding that the extra information may be allowing younger siblings to make better choices.

Also, while siblings may be a valuable source of support while in college, we find no evidence that spillover effects across major/institutions are driven exclusively by increased benefits of attending college simultaneously. We observe that students are willing to follow their older siblings into their majors/institution combinations even when, based on their age difference and the duration of the program chosen by the older sibling, they are unlikely to be enrolled in college together. Moreover, we find no evidence that attending an older sibling's college can increase graduation rates for students, ruling out any positive spillover effect on graduation outcomes.

⁴The same paper estimates a 90% effect of having a sibling who attends or attended the same college, somewhat larger than the effects we find, but in the same order of magnitude. Beyond differences in context, their larger estimates for sibling effects may in part be explained by correlated effects that are not accounted for in their analyses.

We can also discard any cost reduction associated with attending college together, as siblings in Chile do not get any type of tuition discount, nor do they receive any special treatment in terms of financial aid when attending the same college. Other cost reduction could include reduced housing costs. However, unlike college students in the U.S., Chilean students who live outside their parents' home are a minority (38% in our sample).⁵ Based on previous finding, we conclude that spillover effects across major/institution are not driven primarily by increased benefits of attending college together, or information effects, but rather other mechanisms. This could include role model effects, or younger siblings assigning a symbolic or expressive value to attending the same major/institution as their older siblings.

While spillover effects across major/institution combinations are strong and remain stable across different groups of students, these positive spillover effects do not extend directly to attending the same major in a different institution, or a different major within the same institution. As was mentioned above, we find no evidence of positive spillover effects across majors at other colleges; and while we find positive spillover effects across other majors within the same college, these effects seem to restrict to cases where siblings are expected to coincide while in college. As was mentioned above, considerations about the Chilean context point to non-pecuniary rather than pecuniary benefits of attending college simultaneously, as there are no major cost reduction associated with attending college together with ones sibling in Chile.

Higher education choices are high stakes decisions that can have important implications on students' long-term outcomes. More importantly, older siblings can induce students to either high or low-quality major/institution combinations. And while students might derive important non-pecuniary benefits from following their older siblings, this could have important, and potentially negative, effects on their long-term outcomes. Moreover, we are able to reject any positive effect on graduation outcomes of enrolling in the same college

⁵Results remain the same regardless of whether older siblings declare at baseline that they intend to live with or without their parents.

as an older sibling.⁶ Further highlighting that although the consequences of following ones' sibling might be pecuniary, the benefits are likely to be non-pecuniary.

This paper contributes to the literature on siblings and higher education choices. While there are some papers that have reported a positive correlation in siblings' education choices at the tertiary level, this is the first paper to provide causal evidence on the topic. Existing studies include [Goodman et al. \(2015\)](#), who report a high correlation in sibling's higher education choices in the U.S. Also, [Loury \(2004\)](#), estimates that, controlling for a number of variables, African American's college enrollment rates are higher when they have older siblings that have attended college. Exploiting discontinuities in higher education admission rules, we are able to show that even after accounting for all sources of endogeneity, sibling spillovers in higher education choices remain strikingly high.

More causal evidence on siblings' spillovers can be found at the secondary education level. [Joensen and Nielsen \(2017\)](#) use quasi-experimental evidence from Denmark and report that older sibling's access to advanced math and science coursework can alter the coursework choices of younger siblings. Also, in a paper that is methodologically close to ours, [Dustan \(2018\)](#) exploits discontinuities in admissions to selective high schools in Mexico and finds positive sibling spillover effects. Importantly, we are able to show that these spillover effects remain high even at the tertiary level, where the benefits of attending school together might be lower, and the stakes of the decision might be higher.

The paper further contributes to provide a better understanding of the potential mechanisms driving the results. Younger siblings seem to derive benefits from attending the same major/institution as their older sibling, even if they are not enrolled in college together, and even in cases where they are likely to be well informed about the program. Importantly, these benefits do not seem to extend directly to attending the same major in a different institution or a different major in the same institution.

Although previous results refer specifically to siblings, we study a context where students

⁶This can be done by comparing the graduation outcomes of younger siblings who were marginally admitted and marginally rejected from a program within their older sibling's college.

derive few pecuniary benefits from attending college together with other family members. Moreover, we are able to show that results can remain strong even in contexts in which students are unlikely to attend college together. Therefore, results could be thought of as highlighting not just the importance of siblings in higher education choices, but of close relations more generally. Studies looking at peers effects more generally have found mixed results regarding their impact on college choices. [Sacerdote \(2001\)](#) finds that college roommates have no effect on college major choices, [Arcidiacono and Nicholson \(2005\)](#) finds that classmates in medical school have no effect on medical specialization choices, while [De Giorgi et al. \(2010\)](#) finding that classmates in Bocconi University significantly affect major choices for students. Mixed findings do not come as a surprise considering that all these studies look at different sets of peers that might or might not be close to the individual. In this sense, our results speak about the potential magnitude of these effects when individuals are indeed close to one another.

Previous results are also highly relevant from a public policy perspective. On the one hand, results could help explain why we often observe what appear to be non-optimal higher education choices. For instance, [Hoxby and Avery \(2013a\)](#) find that high-achieving students from low-income families often do not apply to selective institutions despite generous financial aid opportunities that make those alternatives less expensive than the ones they actually choose. If social interactions are important, the latter could be partly a result of students in low-income families being less exposed to the experiences of students attending selective colleges. In the long term, given socioeconomic segregation patterns that limit the interaction of low-income students with people from different backgrounds, social spillovers in higher education choices could lead to poverty traps where the poor remain poor because of their low exposure to people choosing alternatives with high economic returns. On the other hand, previous results point to the potential existence of social multiplier effects that, if properly considered, could enhance the effectiveness of policies aimed at improving students' decision-making.

The remainder of this paper is organized as follows. Section II develops the empirical framework for interpreting spillovers in the choice of multiple, unordered alternatives. In Section III we briefly describe the Chilean centralized system of admission to higher education. In Section IV we explain how we construct our data set. Section V presents our multi-cutoff regression discontinuity strategy. Section VI presents our main empirical findings. Finally, Section VII concludes.

II Spillovers in the Choice of Multiple, Unordered Alternatives

The study of sibling spillovers in higher education choices demands a framework for interpreting treatment effects in contexts where both the treatment and the outcome correspond to choices from a set of multiple, unordered alternatives. In this section, we develop such framework and discuss conditions under which identification of spillovers is possible.

Interpretation of causal effects in settings with multiple unordered treatments and a continuous outcome has already been addressed by Kirkeboen et al. (2016), who study the earnings returns to different fields of study in higher education. Our results can be thought of as an extension of their framework to settings with multiple unordered outcomes.

A Setting

Let J be a set containing all available major/institutions (i.e., a specific major offered in a given college) as well as the outside option. Throughout this section, we will consider sibling pairs with an older sibling in the margin of admission to major/institution $x \in J$, which we will refer to as the *cutoff major/institution*. We simplify notation by ignoring individual-specific indices.

An older sibling's enrollment in major/institution $k \in J$ is characterized by an indicator function d_k taking the value 1 if she enrolls in major/institution k . This indicator can take

on two possible values, $d_k(0)$ and $d_k(1)$, depending on the value of a binary instrument Z indicating whether the older sibling is offered admission to the cutoff major/institution x . In our empirical application, Z will equal 1 if the older sibling's weighted test score is above the admission cutoff for x , and zero otherwise.⁷ Enrollment of the older sibling can thus be expressed as a function of Z :

$$d_k = d_k(0) \cdot (1 - Z) + d_k(1) \cdot Z, \quad \forall k \in J$$

We impose the restriction that each student enrolls in one and only one alternative (which may be the outside option) regardless of the value of Z , that is, $\sum_{k \in J} d_k(z) = 1$, for $z = 0, 1$.

The choices of younger siblings are modeled as follows. For any pair of major/institutions $j, k \in J$, we define an indicator function $q_{jk}(z)$ taking the value 1 if the younger sibling chooses major/institution j when the older sibling enrolls in major/institution k , given $Z = z$. Letting y_j be an indicator for whether the younger sibling chooses major/institution j , we can write:

$$y_j = y_j(0) \cdot (1 - Z) + y_j(1) \cdot Z, \quad \forall j \in J$$

where the potential value of y_j given $Z = z$ is:

$$y_j(z) = \sum_{\forall k \in J} q_{jk}(z) \cdot d_k(z), \quad \forall j \in J, z = 0, 1$$

B Spillovers within major/institutions

The setting laid out above is flexible enough to allow for spillovers to take place either within or across major/institutions. This means that having an older sibling enroll in major/institution j may not just affect a younger sibling's choice of j but also her choice of other programs.

⁷Note that we are considering a situation with only one instrument, Z . This stands in contrast to the framework developed in [Kirkeboen et al. \(2016\)](#), which presumes the existence of one instrument for each alternative. We believe that the case with a single instrument corresponds better to our empirical setting, where an older sibling is typically in the margin of admission to only one major/institution.

Let us leave aside for the moment the possibility of spillovers taking place across major/institutions, and suppose that we are interested in studying the effect of having an older sibling enrolled in the cutoff major/institution x (i.e., $d_x = 1$) on the probability that the younger sibling chooses x (i.e., $y_x = 1$). For that purpose, we may want to estimate β in the following regression, using Z as an instrument for d_x :

$$y_x = \alpha + \beta \cdot d_x + \varepsilon \quad (3.1)$$

We adopt the following standard assumptions for a meaningful interpretation of β :

Assumption 1 (*Independence*) $d_k(z), q_{jk}(z) \perp Z \quad \forall j, k \in J; z = 0, 1$

Assumption 2 (*Relevance*) $E[d_x(1) - d_x(0)] \neq 0$

Assumption 3 (*Exclusion*) $q_{jk}(z) = q_{jk} \quad \forall j, k \in J; z = 0, 1$

The first assumption requires the instrument to be independent of potential enrollment of older siblings and potential choices of younger siblings.⁸ Assumption 2 requires Z to effectively change enrollment in the cutoff major/institution x for at least some older siblings. The exclusion restriction formalized in Assumption 3 requires the instrument Z to affect the choices of the younger sibling only through its effect on the older sibling's enrollment. This rules out the possibility of any direct effects of an admission offer to the older sibling on the choices of the younger sibling. If, for instance, we believe that effects of Z on y_x operate through changes in younger siblings' expectations or confidence boosts, then Assumption 3 requires these mechanisms to operate only when admission offers actually change older siblings' enrollment. With this assumption, the individual-level effect of an older sibling's admission offer to the cutoff major/institution on the younger sibling's choice of x can be expressed as:

⁸Although in our regression-discontinuity application this independence will only hold after conditioning on functions of the running variable at either side of the admission cutoff, we ignore this issue for now and treat Z as if it were a random, independent variable determining who is offered admission to x

$$\tau = y_x(1) - y_x(0) = q'_x \Delta d,$$

where $q_x = (q_{x1}, \dots, q_{xJ})'$, $d(z) = (d_1(z), \dots, d_J(z))'$ for $z = 0, 1$, and $\Delta d = d(1) - d(0)$. Note that the effect of Z on y depends as much on the spillover structure contained in q_x as on the response of the older sibling to an admission offer to x captured in Δd . As a result, even if we are able to identify $E[\tau]$, our ability to say anything meaningful about $E[q_x]$ will necessarily depend on the assumptions we make about Δd . We thus adopt the following additional assumptions:

Assumption 4 (*Monotonicity*) $d_x(1) \geq d_x(0)$

Assumption 5 $d_k(1) \leq d_k(0) \quad \forall k \in J - \{x\}$

Assumption 4 requires an admission offer to major/institution x never to dissuade older siblings from enrolling in x . Note that assumptions 1 through 4 are analogous to the standard conditions for interpreting instrumental variables estimates as local average treatment effects, or LATEs (Imbens and Angrist, 1994). To these conditions, we add Assumption 5, requiring an admission offer to major/institution x not to increase an older sibling's likelihood of enrolling in major/institutions different from x .

Taken together, monotonicity and Assumption 5 restrict the ways in which the instrument Z affects the complete vector of enrollment of the older sibling, $(d_k)_{\forall k \in J}$. Intuitively, under these assumptions, an admission offer to major/institution x can either i) leave the older sibling's enrollment unaffected, or ii) induce the older sibling to enroll in x instead of a counterfactual major/institution $k \neq x$.

With these assumptions at hand, we now ask about the theoretical interpretation of the instrumental variables estimates of β in equation (3.1) using Z as the instrument. The following proposition formalizes the main result of this section.

Proposition 1. *Let β_{IV} be the probability limit of the IV estimate of β in regression (3.1) using Z as the instrument. Then, if Assumptions 1-5 hold:*

$$\beta_{IV} = \sum_{k \neq x} E[\beta_k | \Delta d_x = 1 \wedge \Delta d_k = -1] \cdot \frac{\omega_k}{\omega}, \quad (3.2)$$

where $\beta_k = q_{xx} - q_{xk}$, $\Delta d_k = d_k(1) - d_k(0)$, $\omega = Pr(\Delta d_x = 1)$ and $\omega_k = Pr[\Delta d_k = -1]$.

Proof. See Appendix A. □

It is convenient to think of compliers as older siblings for whom $\Delta d_x = 1$, that is, students who are encouraged to enroll in major/institution x when $Z = 1$ but would otherwise enroll in a different major/institution. Depending on the specific counterfactual major/institution, these compliers can be of $J - 1$ possible types. We refer to an older sibling with $\Delta d_x = 1$ and $\Delta d_k = -1$ as a k -complier. Proposition 1 tells us that, under assumptions 1-5, IV identifies a weighted average of LATEs across k -compliers, with weights corresponding to the proportion of compliers who are of each type.⁹

On the one hand, this result tells us that IV correctly identifies an average effect on the probability of choosing the cutoff major/institution x of having an older sibling who enrolled in x induced by a marginal admission offer. On the other hand, it reminds us that a careful interpretation of IV estimates should take into account the fact that individual effects are heterogeneous not only because of variation in $\{\beta_k\}_{k \in J}$ among younger siblings of compliers, but also because compliers can differ in terms of their counterfactual major/institutions.

This result bears some resemblance with the results of Kirkeboen et al. (2016). However, while in their context the IV estimate does not have a meaningful economic interpretation, in our context it does. Therefore, we do not need to use information on older sibling's next-best alternatives to get a measure of the effect of having an older sibling enroll in a major/institution on younger sibling's choices.

⁹Note that under assumptions 4 and 5 we have $\sum_{k \in J} \omega_k = \omega$, and thus the weights in (3.2) sum up to 1.

C Spillovers across major/institutions

Let us now focus on spillovers taking place across major/institution combination. In principle, the results of the previous section could easily be extended to study the effects of an older sibling's enrollment in x on the probability that the younger sibling chooses an alternative $j \neq x$. It would be enough to estimate a regression of y_j on d_x using Z as the instrument, and interpret the results along the lines of Proposition 1. Doing this for every $j \neq x$, however, would be as tedious as it would be uninteresting.

It is natural to expect spillovers to be especially relevant across programs that are similar in some way. For instance, a student's enrollment in an engineering degree may have a positive effect on her younger sibling's interest for all math-intensive degrees. Similarly, to the extent that commuting or housing costs can be shared among siblings attending the same college, students may have incentives to follow their older siblings into their college, even if not to the same major. Based on this idea, we study how having an older sibling induced to enroll in the cutoff major/institution x affects her younger sibling's choice of any major/institution j in the same institution as x . Of course, the results will also be valid for the study of spillovers within majors, fields, campuses, cities, or any other possible categorization of major/institutions.

Throughout, we will refer to the college where the cutoff major is offered as the cutoff college. Let c_j be a binary indicator for whether major j is offered in cutoff college. Our outcome of interest will be y_x^c , an indicator for whether the younger sibling chooses any major in the cutoff college. As before, we can write y_x^c in terms of potential outcomes as:

$$y_x^c = y_x^c(0) \cdot (1 - Z) + y_x^c(1) \cdot Z,$$

where for $z = 0, 1$ we have $y_x^c(z) = \sum_{\forall k \in J} q_{xk}^c \cdot d_k(z)$, and $q_{xk}^c = \sum_{\forall j \in J} q_{jk} \cdot c_j$ is an indicator for whether the younger sibling enrolls in any major in the cutoff college when the older sibling enrolls in major/institution k .

The symmetry with the case of spillovers within major/institutions makes it straightforward to see that under assumptions 1-5, an IV estimate of the effect of d_x on y_x^c using Z as the instrument would identify a weighted average of parameters $E[\beta_k^c | \Delta d_x = 1, \Delta d_k = -1]$ across k -compliers, where $\beta_k^c = q_{xx}^c - q_{xk}^c$.

Here too, the IV estimate of β_k^c has a meaningful economic interpretation. Allowing us to estimate the effect of having an older sibling enroll in major/institution x on the younger sibling's probability of choosing any major within that college. However, we might be concerned by the fact that some compliers are not compliers in terms of college. An older sibling with $\Delta d_x = 1$ and $\Delta d_k = -1$ for some k in the cutoff college would in practice be encouraged by the admission offer to move from one major to another in the same college. For such a complier, i.e., for an older sibling with $\Delta d_x = 1$ but $\Delta d_x^c = \sum_{j \in J} \Delta d_j \cdot c_j = 0$, it might be reasonable to assume that Z does not affect the younger sibling's likelihood of choosing a major/institution in the cutoff college. We thus take one step further and assume:

Assumption 6 (*Restricted spillovers*) $\beta_k^c \cdot c_k = 0 \quad \forall k \in J$

That is, for any major/institution k in the cutoff college, inducing the older sibling to enroll in x instead of k does not affect the probability that the younger sibling chooses a major/institution in the cutoff college. Note that this assumption still leaves space for spillovers in the choice of major/institutions within the cutoff college.

With this additional assumption, we ask about the interpretation of the IV estimate of β^c in the following regression:

$$y_x^c = \alpha^c + \beta^c \cdot d_x^c + \varepsilon^c, \quad (3.3)$$

where Z is used as an instrument for $d_x^c = \sum_{j \in J} d_j \cdot c_j$.

Proposition 2. *Let β_{IV}^c be the probability limit of the IV estimate of β^c in regression (3.3) using Z as the instrument. Then, if Assumptions 1-6 hold:*

$$\beta_{IV}^c = \sum_{\forall k \in J: c_k = 0} E[\beta_k^c | \Delta d_x^c = 1 \wedge \Delta d_k = -1] \cdot \frac{\omega_k}{\omega_c}, \quad (3.4)$$

where $\omega_c = \Pr(\Delta d_x^c = 1)$, and the rest of the terms are defined as before.

Proof. See Appendix B. □

This proposition tells us that, under Assumptions 1-6, the IV estimate of the effect of d_x^c on y_x^c using Z as the instrument can be interpreted as a weighted average of LATEs across college compliers of type k , where the weights correspond to the proportions of college compliers who are of each type. If we abstract from the heterogeneity emerging from difference in LATEs across complier types, this result means that we can identify the average effect on the probability of choosing a major/institution in the cutoff college of having an older sibling induced to enroll in x instead of a major/institution k in a different college.

In practice, in our analysis we report estimates of β_k^c using Z as an instrument for d_x and d_x^c . Using information on older sibling's next-best alternatives we are able to show that β_k^c is close to zero when the older sibling's counterfactual is a different major within the same college, granting validity to Assumption 6.

III Institutional Setting

The Chilean postsecondary education sector consists of 60 universities that offer college major/institutions and 122 institutions that offer technical major/institutions. College degrees typically take 5 years to complete on time. Of the total number of universities, 33 participate in a centralized admission system called SUA (for *Sistema Único de Admisión*, or Unified System of Admission).¹⁰ Universities that do not participate in this admission system are predominantly private and typically serve lower-scoring students. The 33 universities that participate in SUA are all not-for-profit, but can be public, private, or private-parochial. These universities span a wide range of selectivity levels.

Students applying to these 33 institutions must take an SAT-like standardized test called PSU (for *Prueba de Selección Universitaria* or University Selection Test.) Students sign up

¹⁰Before 2012, only 25 Universities participated in the centralized admission system.

online to take the PSU during their last year of high school, and everyone must take the test on the same day by the end of the academic year in December. There is only one chance to take the test each year. All students take exams in mathematics and language, and they can choose whether to take optional tests in science and history. Scores for these tests are scaled to a distribution with range 150 to 850 and a mean and median of 500. Entrance exam scores, along with high-school GPA, and GPA ranking¹¹ are the primary components of the composite scores used for postsecondary admissions.

After taking the PSU and being informed of their test scores, students submit their applications to the system using an online platform. As in many other postsecondary education systems, students in Chile apply directly to specific majors within postsecondary institutions. As a point of reference, in 2017, students could choose from a total number of 1,477 majors in institutions participating in SUA. Each year, institutions must define ex-ante the weights each program will give to the different sections of the PSU as well as to high school GPA and GPA ranking. For instance, the composite admission score to a medicine major at *Pontificia Universidad Católica de Chile* gives a high weight to the science section of the PSU and no weight to the history section. Let s_i^ι be the score obtained by student i in PSU section ι (e.g., math, history, or GPA). The program-specific weighted score of student i applying to program j is computed as:

$$s_{ij} = \sum_{\forall \iota} s_i^\iota \cdot \alpha_j^\iota,$$

where α_j^ι is the weight given to PSU section ι in major/institution j , with $\sum_{\forall \iota} \alpha_j^\iota = 1$ for any major/institution j . Note that, because α_j^ι vary across major/institutions, the same student may have different weighted scores for different programs. The weights are public information and thus applicants can know beforehand what their weighted scores would be for each available major/institution.

¹¹The GPA ranking was introduced in 2012 as a variable for admission. It measures a student's GPA ranking variable relative to previous cohorts' average GPA

In their applications, students submit a list with up to ten programs ranked from most to least preferred.¹² Students have an incentive to rank programs correctly, meaning that they should not list a less-preferred choice over a more-preferred choice. However, they may incorporate admission probabilities when deciding which options to list, as they are capped at ten options.

Once students submit their applications, the system takes their rankings of alternatives, their program-specific scores, and the number of available seats by program, and implements a *deferred acceptance* assignment algorithm (Gale and Shapley, 1962) to determine which students are offered admission to each major/institution. The algorithm generates program-specific admission cutoffs such that (i) each student is offered admission to his highest-ranked program for which his program-specific weighted score is equal to or above the program-specific admission cutoff (if any), and (ii) the number of students assigned to each program is equal to or less than the number of available seats for that program. While students apply with some knowledge of where they might be admitted, cutoff scores vary unpredictably from year to year due primarily to shocks in demand. Student's inability to precisely predict cutoff scores is consistent with the imprecise control condition required for unbiased regression discontinuity estimation (Lee and Lemieux, 2010).

The admission process has two rounds. During the first round, students receive at most one admission offer and decide whether to enroll, remain in the waitlist for a more-preferred major/institution from which they were rejected, or withdraw from the application process. The seats that remain empty after the first round are then allocated in a second round of offers. These second offers are generated following the same mechanism as the first round. In March of the following year, enrolled students begin their studies in their major/institutions. If students want to change to a different major/institution they usually need to wait a whole year and participate in the next admission process on equal terms with other applicants.

¹²Up until 2011 students could submit only 8 options, but as of 2012 they can submit up to ten choices

IV Data and Sample Construction

A Data

In the analysis, we focus on pairs of successive siblings, where the older sibling applied to postsecondary education between 2004 and 2016, and the younger sibling completed high school before or in 2017. We identify siblings following two complementary strategies. The first strategy uses surnames reported by high school institutions and contained in administrative records from Chile’s secondary education system. Chile, as most of Latin American countries, follows the Spanish naming tradition where a person receives two surnames, the first corresponding to the father’s first surname, and the second to the mother’s first surname. Our strategy is based on the idea that, within a given high school institution, it is highly unlikely that two students who are not siblings will share the same pair of surnames, in the same order. We thus classify two students as siblings if i) they share the same pair of surnames, in the same order, and ii) they go to the same high school institution. In order to reduce the probability of incorrectly classifying two students as siblings, we restrict our attention to pairs of students born at least nine months and at most 10 years apart from each other. Furthermore, we drop from our sample surname pairs with a frequency above 100 in the same year. We have data on surnames for all the cohorts of students enrolled in any primary or secondary education institution in Chile (either public or private) between 2004 and 2017.

Our second strategy is based on a unique national identification number (NID) that is assigned to every Chilean citizen at birth as well as to foreign residents. Specifically, we use mother’s NIDs reported by students in their online registration to the PSU, and classify two students as siblings if they both provided the same NID for their mother. This method requires that both siblings register for the PSU, and that they both provide valid NIDs. The former is not much of a problem considering that older siblings who are in the margin of being admitted to a given major/institution have all registered for the PSU, and that

younger siblings whose older siblings have registered for the PSU have a 99% probability of registering themselves. The latter, however, is somewhat more restrictive considering that approximately 75% of students who register for the PSU provide valid NIDs.¹³

Both methods have advantages and disadvantages. On the one hand, identification of siblings based on surnames may wrongly identify two individuals who share the same last-names, but are non-related, as siblings. On the other hand, identification of siblings based on mother's NIDs, while more precise, only allows us to identify sibling pairs in which both siblings provide valid information on their mothers' NIDs. This could lead to attrition bias if the oldest sibling's admission outcome has an impact on our probability of identifying the younger sibling.

Fortunately, the fact that we have two different methods for identifying siblings allows us to check the quality of each. To assess the quality of the first method we use the sub-sample of students identified under both methods and check the probability that two students who share the same surnames provided the same NID for their mother. Under the premise that siblings who provide the same NID are well identified, this exercise suggests that 93% of students who share the same surnames are in fact siblings. To assess the quality of the second method, we use data on siblings identified under the first method and estimate the impact of the treatment on the younger sibling's probability of registering for the PSU and providing a valid NID. Our results show no effect of the treatment on our probability of identifying younger siblings. Estimates on Section A also confirm that there is no evidence of a jump in the density of observations around the discontinuity, or of a difference in observable characteristics for individuals who are above or below the cutoff, which would be the case in the presence of attrition bias.

In the period from 2004 to 2016, 2,526,246 students registered for the PSU. Assuming a family size of 2, which is close to the national average, this means that roughly 1,200,000 of these students should have a younger sibling. Using both of our methods we are able to

¹³Valid NIDs have an internal numerical structure that can be easily verified with an algorithm.

identify younger siblings for 636,252 students. Section C describes more in detail the characteristics of the sample for which we are able to identify younger siblings, and shows how they compare to the characteristics of the general population. Our final sample is composed of 81,631 older siblings whose weighted score for at least one of their listed major/institutions is close to the admission cutoff for that major/institution. Of these, 20,960 are identified exclusively from mothers' NIDs, 36,684 are identified exclusively from surnames, and 23,987 are jointly identified by both methods. Importantly, the possibility that some student pairs are incorrectly identified as siblings should bias our estimates towards zero, working against our hypothesis of sibling spillovers in higher education choices. In order to maximize statistical power, our results are based on the full sample of siblings, but the main results do not differ considerably when we focus on sibling pairs identified by one method or the other.¹⁴ Section B describes more in detail how this final sample is constructed.

We link data on siblings to detailed administrative records on the complete process of admission to higher education as well as previous high school records. Both for older and younger siblings, we are able to obtain data on i) a short online survey taken at PSU registration, containing basic socioeconomic data, ii) PSU test scores and detailed high school GPA records, iii) the ranking of major/institutions provided in the application, in order of preference, and iv) students' actual enrollment and graduation. We have data on enrollment for institutions participating in the SUA admission system between 2004 and 2017, and data on enrollment for institutions not participating in the SUA admission system between 2007 and 2017. Because of the latter, when we estimate the younger sibling's probability of enrolling in an institution not participating in the SUA admission system, we restrict our sample to younger sibling graduating from high school in or after 2006. This is the case, for example, when we estimate the younger sibling's probability of enrolling in the same major as their older sibling within any institutions.

¹⁴These results are available upon request.

B Sample Construction

Our sample is composed of successive sibling pairs in which the older sibling has a weighted score for at least one of his listed major/institutions that is close to the admission cutoff for that program.¹⁵ We define the admission cutoff for major/institutions j , denoted by c_j , as the minimum weighted score among students who enroll in major/institutions j , that is:¹⁶

$$c_j = \min_i \{s_{ij}\} \quad s.t. \ i \text{ enrolls in } j$$

It is also convenient to define the standardized weighted score \tilde{s}_{ij} as the distance between a student’s weighted score for major/institution j and the admission cutoff for j , that is, $\tilde{s}_{ij} = s_{ij} - c_j$.

Our identification strategy rests on the idea that the probability of an older student i enrolling in major/institutions j increases discontinuously around $\tilde{s}_{ij} = 0$. As several papers have pointed out, however, this threshold may not be relevant for some of the programs included in a student’s application (e.g., [Abdulkadiroğlu et al., 2014](#)). Take for instance the case of an older sibling i who ranks first major/institution k with very low selectivity, followed by major/institution j with very high selectivity. For this student, crossing j ’s admission cutoff would have no effect on assignment to j , because the less selective but preferred major/institution k is within reach when $\tilde{s}_{ij} = 0$. In this case, including i ’s application to j in our dataset would reduce the strength of our first stage, thus lowering statistical power and increasing the risk of weak instruments bias.

We deal with this issue following in spirit [Dustan \(2018\)](#), and eliminating from our

¹⁵We exclude non-consecutive siblings from our sample because including them might lead to an over estimation of sibling spillovers. Consider for instance a family with three siblings, and suppose that direct spillovers are homogeneously 10%. The causal effect of having the oldest sibling enroll in the cutoff major/institution on the likelihood that the youngest sibling chooses the cutoff major/institution will be 11%, including a direct effect of 10% and an indirect effect (through the middle sibling’s enrollment) of $10\% \times 10\% = 1\%$.

¹⁶An alternative would be to define cutoffs as the weighted score of the last student to be offered admission to j in the first round of admissions. Results under this alternative definition remain essentially the same and are available from the authors upon request.

sample any application to a major/institution k by an older sibling i if there exists a major/institution j such that both:

- i) i ranks j above k , and
- ii) j is *relatively less selective* than k from i 's perspective,

where relative selectivity is defined as follows:

Definition 1 (*Relative Selectivity*) Let $\phi_{ij} = \frac{\bar{s}_{ij}}{\sqrt{\sum_{\forall l} (\alpha_j^l)^2}}$ be the euclidean distance between i 's vector of scores, $(s_i^t)_{\forall l}$, and the admission line for j defined as $C_j = \{(s^t)_{\forall l} : \sum_{\forall l} s^t \alpha_j = c_j\}$. Then major/institution j is said to be *relatively more selective* from i 's perspective than major/institution $k \neq j$ if and only if $\phi_{ij} < \phi_{ik}$.

It is easy to check that for the special case where major/institutions j and k assign the same weights to each section of the test, relative selectivity of j and k depends exclusively on the comparison between c_j and c_k . Approximately 55% of the applications survive the elimination process described by i) and ii).

We also exclude from our sample applications to major/institutions that are not oversubscribed. In practice, we consider a major/institution j to be oversubscribed if at least one student among j 's applicants ends up being assigned to an alternative that is less preferred.

C Sample Description

Table 3.1 presents summary statistics for the older siblings in the resulting sample, and shows how they compare to the sample of older siblings that we are able to identify, and to the general population of high school graduates who signed up for the standardized admission test in the 2004 to 2016 period.

About 50% of students who sign up for the PSU are females, and roughly 40% live in the capital. Their households are composed of 4.4 individuals, where approximately 1.2 work. Two thirds of these students report the father to be the head of household and one third

report the mother to be the head of household. Students report a monthly family income of 856 USD. Approximately a quarter of these students have mothers with tertiary education, and one third have fathers with tertiary education. Two thirds of them have a father that works full-time, but only a third have a mother that works full-time. Students have a GPA of 5.6, and score slightly less than 500 points on the math and language PSU.

The sample of students for whom we are able to identify younger siblings is very similar to the general population. They have, however, slight higher socioeconomic characteristics. They are more likely to report the father to be the head of household, they report higher monthly family incomes, they have more educated parents, and their fathers are more likely to work full-time. They also score slightly higher on the PSU tests.

Compared to previous populations, the sample that we use in the analysis has a higher socioeconomic status and is more academically advantaged. This makes sense considering that the older siblings in this sample are close to the cutoff score for at least one of the oversubscribed major/institutions in the centralized system of admission. These students are more likely to report the father to be the head of household, report higher monthly family incomes, have more educated parents, and are more likely to have mothers or fathers that work full time. They also have higher GPAs and perform much better in the language and math PSU test.

V Multi-cutoff RD Strategy

In this section, we briefly discuss how our empirical strategy exploits the exogenous variation originated in discontinuous assignment rules to estimate the causal impact of older siblings' enrollment decisions on higher education choices of younger siblings.

The identification results of Section II presume the availability of an instrument Z randomly defining whether or not an older sibling receives an admission offer from a given *cutoff major/institution* x . Our empirical strategy departs from this abstract setting in two ways.

First, rather than random admission, our identification will rest on quasi-experimental variation generated by discontinuities in assignment probabilities around program-specific admission cutoffs. Second, our estimates will be obtained from pooling together older students who are in the margin of admission to different major/institutions, making our design a case of multi-cutoff regression discontinuity ([Cattaneo et al., 2016a](#)).

Our reduced-form results are based on the following regression specification:

$$y_{ijt} = \pi_0 + \pi_1 \cdot \tilde{s}_{ijt} + \pi_2 \cdot (Z_{ijg} \times \tilde{s}_{ijt}) + \tau \cdot Z_{ijt} + \mu_{jt} + \varepsilon_{ijt}, \quad (3.5)$$

where the outcome y_{ijt} is a binary variable indicating whether younger sibling i ever enrolled in major/institution j (or alternatively in j 's college or major) after the older sibling applied in year t , \tilde{s}_{ijt} is the older sibling's standardized weighted score (i.e., $\tilde{s}_{ijt} = s_{ijt} - c_{jt}$), and Z_{ijt} is a cutoff-crossing indicator (i.e., $Z_{ijt} = 1 \iff \tilde{s}_{ijt} \geq 0$). Following a standard practice in multi-cutoff RD studies, we include program-by-year fixed effects which is the level of variation of admission cutoffs. Our parameter of interest is τ which, assuming continuity in the conditional probabilities of potential choices around admission cutoffs, captures the causal effect of an older sibling's marginal admission to j on the probability that the younger sibling chooses j . The model is estimated by weighted least squares, using a triangular kernel centered at $\tilde{s}_{ijt} = 0$ with bandwidth $h = 50$,¹⁷ and clustering standard errors at the sibling pair level.

Our IV results, on the other hand, are based on the following structural equation:

$$y_{ijt} = \delta_0 + \delta_1 \cdot \tilde{s}_{ijt} + \delta_2 \cdot (Z_{ijt} \times \tilde{s}_{ijt}) + \beta \cdot d_{ijt} + \eta_{jt} + \epsilon_{ijt}, \quad (3.6)$$

where d_{ijt} takes the value 1 if older sibling i ever enrolled in j (or alternatively in j 's college or major) after applying in year t (but before the year in which the younger sibling

¹⁷Optimal bandwidths computed as in [Imbens and Kalyanaraman \(2012b\)](#) range from 26 to 70 depending on the outcome. We decided to use a single bandwidth of $50 \simeq 0.84 \times s.d.(\tilde{s}_{ijt})$ for all our specifications. All our results are robust to alternative bandwidth definitions.

applied). This model is estimated by two stages least squares, using Z_{ijt} as the instrument for d_{ijt} , and weighting observations using a triangular kernel centered at $\tilde{s}_{ijt} = 0$ with bandwidth $h = 50$. Sibling spillovers will be captured by our estimates of β , which will be interpreted in the light of the identification results presented in Section II.

In an effort to characterize heterogeneous spillovers and uncover some of the underlying mechanisms, we will present estimates of β_0 and β_1 in the following structural regression:

$$\begin{aligned}
y_{ijt} = & \delta_0 + \delta_1 \cdot \tilde{s}_{ijt} + \delta_2 \cdot (Z_{ijt} \times \tilde{s}_{ijt}) + \delta_3 \cdot W_{ijt} + \delta_4 \cdot (\tilde{s}_{ijt} \times W_{ijt}) + \\
& \delta_5 \cdot (Z_{ijt} \times \tilde{s}_{ijt} \times W_{ijt}) + \beta_0 \cdot d_{ijt} + \beta_1 \cdot (d_{ijt} \times W_{ijt}) + \\
& \eta_{jt} + \epsilon_{ijt},
\end{aligned} \tag{3.7}$$

where W_{ijt} is a covariate that is not affected by assignment and may vary across sibling pairs, major/institutions and time. The model is estimated by two-stages least squares using Z_{ijt} and $Z_{ijt} \times W_{ijt}$ as instruments.

VI Results

A RD Validation

We begin by presenting standard tests of the validity of our RD strategy. First, we perform balancing checks to examine whether individuals just above and just below the cutoff are similar in terms of their baseline observable characteristics. We focus on a set of socioeconomic variables reported by the older sibling, including family size, monthly family income, parents' education, and parents' work status. Large and significant discontinuities in the conditional means of these variables at the cutoff could be taken as an indication that potential choices of younger siblings may also be discontinuous at the cutoff, thus violating the exclusion restriction.

Figure 3.1 displays binned scatter plots with group-means of observable characteristics in the vertical axis and group-means of standardized admission scores in the horizontal axis. A visual inspection of these plots suggests that conditional means change smoothly across admission thresholds. The results in Table 3.2 confirm the visual analysis. The table reports differences in means between students who were marginally assigned to and marginally rejected from the cutoff major/institution, estimated from a specification analogous to (3.5), where the baseline characteristic is used as the dependent variable. Coefficients are all small in magnitude and precisely estimated indicating that students at either side of the cutoff are very similar to each other. Although we do find that students above the cutoff are slightly more likely to have mothers that work part-time, this is not surprising given the large number of outcomes being considered. Most importantly, all coefficients are small in magnitude, indicating that students close to the cutoff are comparable in terms of their baseline characteristics.

Manipulation of PSU scores is highly implausible, not only because of the institutional setting, but also because students do not know ex-ante what the cutoff score will be for a given major/institution. Still, as we explained in Section A, there could be attrition bias because of how we identify siblings. To check for this possibility, as well as for any signs of manipulation, we test for a discontinuity in the density of the standardized weighted score around the cutoff. Figure 3.2 shows a non-parametric representation of this density at both sides of the cutoff. We find no visible sign of a discontinuity in the density around the cutoff, something that is confirmed by manipulation testing based on Cattaneo et al. (2017).

B First Stage

We continue by showing evidence of the relevance of admission cutoffs for older siblings' assignment and enrollment. We say that an older sibling was *assigned* to major/institution j if he qualified for admission to j and ranked it above all other major/institutions for which he qualified. On the other hand, an older sibling will be said to have *ever enrolled* in j if

he enrolled in j between the year immediately after his application and the year before his younger sibling is expected to enter college.¹⁸

Figure 3.3 pools together all the applications that meet the restrictions outlined in section B and illustrates how crossing the cutoff affects older siblings': (i) probability of being assigned to the cutoff major/institution, (ii) probability of ever enrolling in the cutoff major/institution, (iii) probability of ever enrolling in the cutoff's major, and (iv) probability of ever enrolling in the cutoff's college. The data are normalized so that zero on the horizontal axis represents a weighted admission score that is equal to the admission cutoff for the cutoff program. Figure 3.3 (a) verifies that the probability of being assigned to the cutoff major/institution is zero for applications to the left of the admission cutoff, and jumps from zero to one at the cutoff. This probability falls monotonically for weighted scores to the right of the cutoff as higher-scoring students are assigned to more-preferred major/institutions.

The effects of assignment to the cutoff major/institution on the older sibling's enrollment are illustrated in the remaining three plots of Figures 3.3. Figure 3.3 (b) shows the effect of crossing the cutoff on the older siblings' probability of ever enrolling in the cutoff major/institution. Note that this probability is slightly above zero to the left side of the cutoff. This is the consequence of older siblings retaking the standardized test in subsequent years and reapplying to the cutoff major/institution. The probability of enrolling in the cutoff major/institution jumps to about 60% at the cutoff, and starts falling for higher scores just as it happens with the probability of assignment to the cutoff program. Figures 3.3 (c) and (d), show analogous analysis for the probabilities of ever enrolling in a program in the same college or major as the cutoff major/institution. We refer to these as the *cutoff college* and the *cutoff major*. Although students with weighted scores to the left of the cutoff don't qualify for the cutoff major/institution, they may qualify for enrollment in a less selective program in the cutoff college or in the cutoff major. Still, the plots show that crossing the

¹⁸In practice, because the timing of the younger siblings' application might be endogenous, we use the year after the younger sibling graduates from high school. We tested for a discontinuity in younger siblings' age at high school graduation and found small and statistically insignificant effects. These results are available from the authors upon request.

admission cutoff increases the probability that the older sibling will ever enroll in the cutoff college by approximately 40 p.p., and in the cutoff major by approximately 30 p.p.

C Sibling spillovers in test score performance

Turning to our results, we begin by analyzing the impact of having an older sibling be admitted into a more preferred major/institution on the younger sibling's probability of taking the PSU, as well as her performance on each of the tests and high school GPA. A priori it is not obvious whether having an older sibling enroll in one program versus another should have any effect on younger sibling's academic performance. However, it is important that we test for any effects to make sure that any impact on choices can be attributed to younger siblings changing their decisions at the moment of applying, rather than having them respond to any change by putting more or less effort in their application process.

Results on Table 3.3 show that having an older sibling be admitted into a more preferred program has no effect on the younger sibling's probability of taking any of the PSU tests. On average 98% of students in the control group take the math and language PSU tests, and this number is non-statistically different for students in the treatment group. Also, conditional on taking the PSU and providing information on their GPA, having an older sibling be admitted into a more preferred program has also no effect on the younger sibling's GPA and test score performance.

D Sibling spillovers in the choice of major/institution

We turn now to the central results of this paper, that is, the impact of an older sibling's enrollment in a specific major/institution on the higher education choices of the younger sibling. We begin by discussing spillovers in the choice of major/institution, and leave spillovers in college and major choice for the following section. Specifically, we study the effects of having an older sibling enroll in a given program on the probability that the younger sibling (i) lists that major/institution as his first choice in his application, (ii)

lists that major/institution as any choice in his application, and (iii) ever enrolls in that major/institution.

Figure 3.4 (a) offers a visual display of our results. The plots show non-parametric representations of the probability that the younger sibling applies to or enrolls in the cutoff major/institution, conditional on the value of the standardized admission score. For each of the outcomes, we find clear evidence of large and positive discontinuities at the cutoff, meaning that younger siblings are more likely to choose a major/institution if their older sibling was previously assigned to that program.

These results are confirmed by the estimates presented in Table 3.4. For reference purposes, Panel A shows first stage estimates, i.e., the effect of crossing the admission threshold on the probability that the older sibling ever enrolls in the cutoff major/institution. Panel B presents reduced form estimates of the effect of having an older sibling cross the admission cutoff for a given major/institution on the probability that the younger sibling chooses that program. These results correspond to estimates of τ in equation (3.5), and are the numerical counterpart of the discontinuities shown in Figure 3.4 (a). Finally, Panel C presents two-stages least squares estimates, where a cutoff-crossing indicator is used as an instrument for the older sibling's probability of ever enrolling in the cutoff major/institution. The reported effects correspond to estimates of β in the regression specification of equation (3.5).

Having an older sibling enroll in a given major/institution increases by 1.5 p.p. the younger sibling's likelihood of listing that major/institution as her first choice, by 2.9 p.p. the likelihood that she lists that major/institution as any choice, and by 1.3 p.p. the probability that she enrolls in that major/institution. Compared to baseline probabilities of 1.6%, 6% and 1.5%, these effects are strikingly large, representing respectively increases of 93.8%, 48.3% and 87%.

E Sibling spillovers in the choice of college and major

In order to dig deeper into the nature of sibling spillovers, we study next how older siblings' enrollment affects younger siblings' choice of college and major. The plots in Figure 3.4 (b) and (c) show probabilities of applying to, or enrolling in a major in the cutoff college or in the cutoff major, conditional on standardized admission scores. We observe sharp, positive discontinuities at the admission cutoffs for the case of college, and positive but noisy discontinuities in the case of major, suggesting that younger siblings are likely to follow their older siblings into other majors in their college, but less so into their majors at other colleges.

Table 3.5 shows regression estimates for the case of spillovers in college choice. We consider three different binary outcomes indicating whether: (i) the younger sibling's first choice is a major in the cutoff college, (ii) the younger sibling listed any major in the cutoff college, and (iii) the younger sibling enrolled in a major in the cutoff college. Estimates can be found on Table 3.5 columns 1, 2 and 3. Having an older sibling enroll in a specific major/institution increases the younger sibling's probability of listing any major within that same institution as his first choice in 6.6 p.p., his probability of listing any major within that same institution as any choice in 7.8 p.p., and his probability of enrolling in any major within that same institution in 4.2 p.p. These effects represent a 33%, 21%, and 25% increase in baseline probabilities.

Previous results could be reflecting a positive effect of having an older sibling enroll in a given college on the younger siblings' probability of choosing that college, or a positive effect of having an older sibling enroll in a more preferred program within a given college on the younger siblings' probability of choosing that college. In order to better understand the mechanisms driving these effects, we make use of information on older siblings' next-best alternatives. We estimate the effect on sibling pairs where the older sibling's next-best alternative is a different major within the same college (columns 4, 5, and 6) and where the older sibling's next-best alternative is a different college (columns 7, 8, and 9). Table 3.5 Panel

A shows that crossing the threshold for older siblings whose next-best alternative is a major within the same college increases their probability of enrolling in that major/institution by 0.59 p.p., but has a minor effect on their probability of enrolling in that college (0.09 p.p.). Instead, crossing the threshold for older siblings whose next-best alternative is a major within a different college increases their probability of enrolling in that major/institution by 0.4 p.p., and their probability of enrolling in that college by 0.61 p.p. Two-stage least square estimates in Table 3.5 Panel C show that having an older sibling enroll in the cutoff major/institution has no effect on the younger sibling’s probability of choosing that college when the older sibling’s next-best alternative is another major within the same college. Instead, when the older sibling’s next-best alternative is a different college, having an older sibling enroll in the cutoff major/institution increases the younger sibling’s probability of listing a major within that college as his first choice in 9.4 p.p., listing a major/institution within that college as any choice in 10.5 p.p., and enrolling in a major/institution within that college in 6.1 p.p.

Because results indicate that effects on younger sibling s’ probability of choosing the cutoff college are driven by an increase in the older sibling’s probability of choosing the cutoff college, we can take a step further and estimate the equation in the full sample using the cutoff-crossing indicator as an instrument for the older sibling’s probability of enrolling in the cutoff college (see Section C). These results can also be found on Table 3.5 Panel C which shows how having an older sibling enroll in the cutoff college increases the younger sibling’s probability of listing a major within that college as his first choice in 9.9 p.p., listing a major within that college as any choice in 11.7 p.p., and enrolling in a major within that college in 6.2 p.p. These effects represent a 59%, 34%, and 42% increase in baseline probabilities and are very close in magnitude to the effects estimated using the sub-sample of older siblings whose next-best alternative was a major within a different college. They are also similar in magnitude to the two-stage least square estimates in the two-sub samples analyzed, when we use the cutoff-crossing indicator as an instrument for the older sibling’s probability of

enrolling in the cutoff college.

Analogous results for sibling spillovers in major choice are reported in Table 3.6. For these analyses, we restrict our sample to sibling pairs where the younger sibling is expected to apply in or after 2008, since only for those years we have data on enrollment in colleges outside SUA (see our discussion in Section A.) Our findings show that we cannot reject a hypothesis of no spillovers in major choice, regardless of whether we look at the whole sample, the sub-sample of sibling pairs in which the older sibling listed the same major within a different college as his next-best alternative, or the sub-sample of sibling pairs in which the older sibling listed a different major as his next-best alternative.

F Understanding the mechanisms

Is it benefits of attending college simultaneously?

The value of attending the same major or college as an older sibling could derive from things such as reduced housing and commuting costs, or simply enjoying each other’s company in campus. If this were the case, we would expect spillovers to be particularly strong for siblings that are expected to coincide in college. If, on the other hand, the value of choosing an older sibling’s college or major derives from the older siblings as a source of valuable information, or even as role models, we would expect spillovers to be present regardless of whether siblings attend college together or not.

To test whether results are particularly strong when siblings are expected to attend college simultaneously, we look at heterogeneous effects by the number of years siblings are expected to coincide in college. For each major/institution in an older sibling’s application, we compute a proxy for expected years of overlap based on (i) the distance in siblings’ high school graduation year, and (ii) program official duration. While in principle an older sibling’s higher education enrollment may affect the younger sibling’s timing of graduation from high school, we find no effects of crossing admission cutoffs on younger siblings’ high

school graduation year, which allows us to treat this proxy as exogenous.¹⁹ The number of years that siblings are expected to coincide while in college is a proxy for how much time they would actually attend together if the younger sibling were to choose that college or major. However, it is a fairly good estimate. As a point of reference, 14% of older siblings who cross the threshold for program admission and are not expected to coincide with their younger sibling are actually enrolled in the cutoff major/institution when the younger sibling applies, compared to 53% of older siblings who are expected to coincide with their younger sibling for four years or more.

The plots in Figure 3.5 show the estimated probabilities of applying to, or enrolling in a given program, major or college, as a function of the number of years of overlap. The gray line shows this probability for younger siblings of marginally rejected students who did not enroll in the cutoff program, college, or major; while the red line shows the probability for younger siblings of students who enrolled in the cutoff program, college, or major. We use the admission offer as an instrument for the probability of enrolling in that program, college, or major. The difference between both lines corresponds to the estimated spillover effect, conditional on the number of years of overlap.

The three plots in the first row of Figure 3.5 show that having an older sibling enroll in the cutoff major/institution increases the likelihood that the younger sibling applies to or enrolls in that major/institution, regardless of the number of years that students are expected to coincide while in college. This is confirmed by columns 1 to 3 of Table 3.7 showing effects for sibling pairs who are not expected to coincide while in college, siblings that are expected to coincide for 1 to 2 year, and sibling who are expected to coincide 3 years or more. Spillovers in the choice of major/institution are found to be strong, but invariant to whether or not siblings are expected to coincide in college, suggesting that the additional value assigned by younger siblings to their older siblings' major/institution does not depend on them attending college together. This finding tends to favor hypotheses such as information sharing or role

¹⁹These results are available upon request.

model effects against those emphasizing benefits of attending a program together with one's sibling.

Results look somewhat different for the case of college choices. The plots in the middle row of Figure 3.5, as well as columns 4-6 of Table 3.7, show that spillovers in college choice are significantly stronger among siblings expected to attend college together for a longer period of time. While we do observe some positive spillovers among siblings with no overlap, these effects are small and only marginally significant for one of the outcomes. Instead, for students who are expected to coincide for three years or more, having an older sibling enroll in a given college increases the younger sibling's probability of listing a major within that college as his first choice in 15 p.p., listing a major within that college as any choice in 17.6 p.p., and enrolling in a major within that college in 8.1 p.p. These effects represent an 97%, 51%, and 51% increase in baseline probabilities. Hence, the benefits for a student of attending the same college as their older sibling seem to depend on them attending college together.

It is worth highlighting that siblings attending college together in Chile do not get any type of tuition discount, nor do they receive any special treatment in terms of financial aid. Also, unlike college students in the U.S., Chilean students who live outside their parents' home are a minority (38% in our sample,) limiting the extent to which attending college together with a sibling may reduce housing costs. Also, results remain the same regardless of whether older siblings declare at baseline that they intend to live with or without their parents.²⁰ Moreover, even though carpooling with one's sibling may significantly reduce commuting costs, students who drive to campus are a small minority. Taken together, these facts suggest that the main advantages of attending college together with an older sibling are non-pecuniary in nature. That is, students may value attending a major in the same college as their older siblings just because it allows them to spend more time together.

Results for major choices once again show no effect of the older sibling's enrollment on

²⁰Results available upon request

the younger sibling's choices. Results are non-statistically significant, regardless of whether siblings are expected to coincide while in college or not.

Is it information?

Information sharing may be an important channel through which older siblings' enrollment affects younger siblings' choices. An older sibling's experience may be a useful source of information that would otherwise be costly to obtain. On the one hand, older siblings can provide students with information about the quality and level of difficulty of their major/institution, as well as a personal appreciation of whether their major or college is appropriate for them. On the other hand, older siblings can be a source of valuable information once younger siblings enroll in their major/institution. For instance, they could help their younger siblings with course selection, or even assist them in their study.

To the extent that students can get access to this type of information from other individuals in their social network, and that the marginal value of information is decreasing in the amount of information, we would expect spillovers to be smaller for younger siblings who have a higher number of peers attending a specific college. We present an empirical test of this hypothesis using data on younger siblings' previous exposure to other students' enrollment experiences. Specifically, for the younger sibling of a student applying to a major in a given college, we compute the share of students in his high school who enrolled in the same college the previous year, and study how spillovers vary for different levels of this variable. We do not present the same exercise for major/institution exposure because, as a result of the large number of programs, enrollment in any given major/institution typically represents a very small share of a high school cohort.²¹

Figure 3.6 summarizes our findings. Even though the probability of the younger sibling choosing the cutoff major/institution and the cutoff college increases with the fraction of

²¹The median share in our sample is zero, and less than 3% of our sample has an exposure to the pivotal major/institution of 5% or more. Therefore, this variable does not help us much in identifying students who are well informed about a specific major/institution.

students in their high school who enrolled in the cutoff college, spillovers (represented by the difference between the red and gray lines) are relatively stable for different levels of exposure. Only for the highest levels of exposure we observe a slight decrease in the size of effects. Still, spillovers in the choice of both major/institution and college remain sizable even among students who graduated from a high school where as much as 1 in 4 students enrolled in the cutoff college the year before they applied. Previous results suggest that information is not the main mechanism driving our findings, or at least not information that can be easily gathered from other sources or social interactions.

How important is the information contained in the signal?

Older siblings can provide students information about high or low-quality major/institution combinations. To analyze whether younger siblings respond differently to information coming from programs that represent a better or worse match for them, we look at heterogeneous effects by the younger sibling’s expected probability of graduating conditional on enrolling in the cutoff program. For each major/institution we predict younger siblings’ probability of graduating when enrolling in that program based on their gender, test score performance, and subject-specific high school GPA ²². These predictions are based on actual graduation rates of students enrolling in those specific programs between 2004 and 2012 (allowing us to observe their graduation outcomes between 6 to 14 years after their initial enrollment). Following [Abadie et al. \(2018\)](#) we implement a leave-one-out approach to compute baseline probabilities of graduating, where we leave both the older and younger sibling out of each prediction.²³

Figure 3.7 summarizes our findings. The probability of the younger sibling choosing the cutoff major/institution is higher the more chances he has of graduating when choosing that degree. However, spillovers represented by the difference between the red and grey lines

²²We have detailed GPA information for years 2007 and 2011 to 2015, which allows us to compute math, language, history, biology, chemistry, and physics GPAs for students through high school.

²³Our variable of interest is the probability of obtaining any degree, meaning that the individual could graduate from the major/institution where he enrolled in the first place or other

are relatively stable for different graduating probabilities. Only for the lowest graduating probabilities we observe somewhat lower spillover effects.

Are results stronger for same-sex siblings?

Finally, we study if sibling spillovers in higher education choices differ for same-sex siblings . Previous studies have found that siblings' spillovers are stronger among same-sex siblings (Goodman et al., 2015). We may expect this to be the case for a number of reasons. For instance, it could be argued that major or college-specific information is more relevant to a younger sibling when it comes from a same-sex older sibling. Furthermore, it may be reasonable to think that same-sex siblings enjoy more of spending time together. Finally, rivalry may be more likely among same-sex students, which may in turn induce younger siblings to seek competition by attending the same major or college.

Our results on heterogeneous effects by siblings' gender composition are shown in Table 3.8. Panel A analyzes whether spillovers look any different for same-sex siblings using the full sample of siblings, while Panels B and C repeat these analyses using the sub-samples of siblings where the younger sibling is female and male, respectively. Overall, we find no systematic differences in spillovers between same-sex and different-sex siblings. This result does not depend on whether the younger sibling is male or female. It does seem, however, that younger brothers are more likely to follow their older siblings into their major/institutions, regardless of their sex.

Can older siblings help students get through college?

Siblings could be a valuable source of academic or emotional support once in college, improving perhaps younger siblings' probability of graduating. To test for this possibility, we once again exploit discontinuous admission rules, but this time analyze the effect of crossing the threshold for the older sibling's major/institution on the younger sibling's graduation outcomes. Thus, comparing the graduation outcomes of younger siblings who were marginally

rejected or marginally admitted to a major/institution in which their older sibling ever enrolled before they applied. We restrict our analysis to younger siblings graduating from high school before 2011, which allows us to look at their graduation outcomes 6 to 13 years after they have graduated from high school.

Results can be found in Table 3.9. Unfortunately, we have too little observations to determine whether enrolling in the same major/institution as an older sibling has any effect on the younger siblings' probability of graduating. However, we are able to reject with a 95% confidence that enrolling in a major/institution within the same college as an older sibling increases younger sibling's graduation rates by more than 3 p.p.

G Placebo test: Spillovers from younger to older siblings?

As a further check on our identification strategy, Table 3.10 presents estimates for the effects of younger siblings' enrollment on older siblings' choices, following the same strategy as before, but inverting the roles of the younger and older siblings. Since younger siblings apply to college after their older siblings, we would not expect to see spillovers going in this direction. Panel A shows that, among older siblings, the shares of compliers by type (estimated from first-stage regressions) are similar to the ones presented above for older siblings. This means that enrollment decisions of younger and older siblings respond similarly to a marginal assignment to the cutoff degree. Reduced-form and two-stages least squares estimates are presented in Panels B and C. As expected, we find no sibling spillovers in the choice of either program, college or major.

VII Conclusion

This paper offers credible empirical evidence of large spillovers in postsecondary education choices from older to younger siblings. Taking advantage of an institutional setting that creates sharp discontinuities in admission offers to particular major/institutions, we find

that younger siblings of students who, induced by a marginal admission offer, enroll in a given program, are 87% more likely to enroll in that major/institution. Similarly, younger siblings of students induced by a marginal admission offer to enroll in a given college are 51% more likely to enroll in that college, almost four times the effects reported in the literature of offering students US \$1,000 college-specific grants.

The magnitude of these effects indicates that attending the same major/institution as an older sibling, or a different major in the same college, is highly valued by applicants to higher education. Understanding why this is the case may provide useful insights for policy design. Our analyses indicate that siblings derive important benefits from attending college simultaneously, which is why younger siblings may be willing to follow their older siblings into their colleges. The latter is probably related to non-pecuniary rather than pecuniary benefits, as siblings in Chile do not get any type of tuition discount and most students in Chile live with their parents while studying.

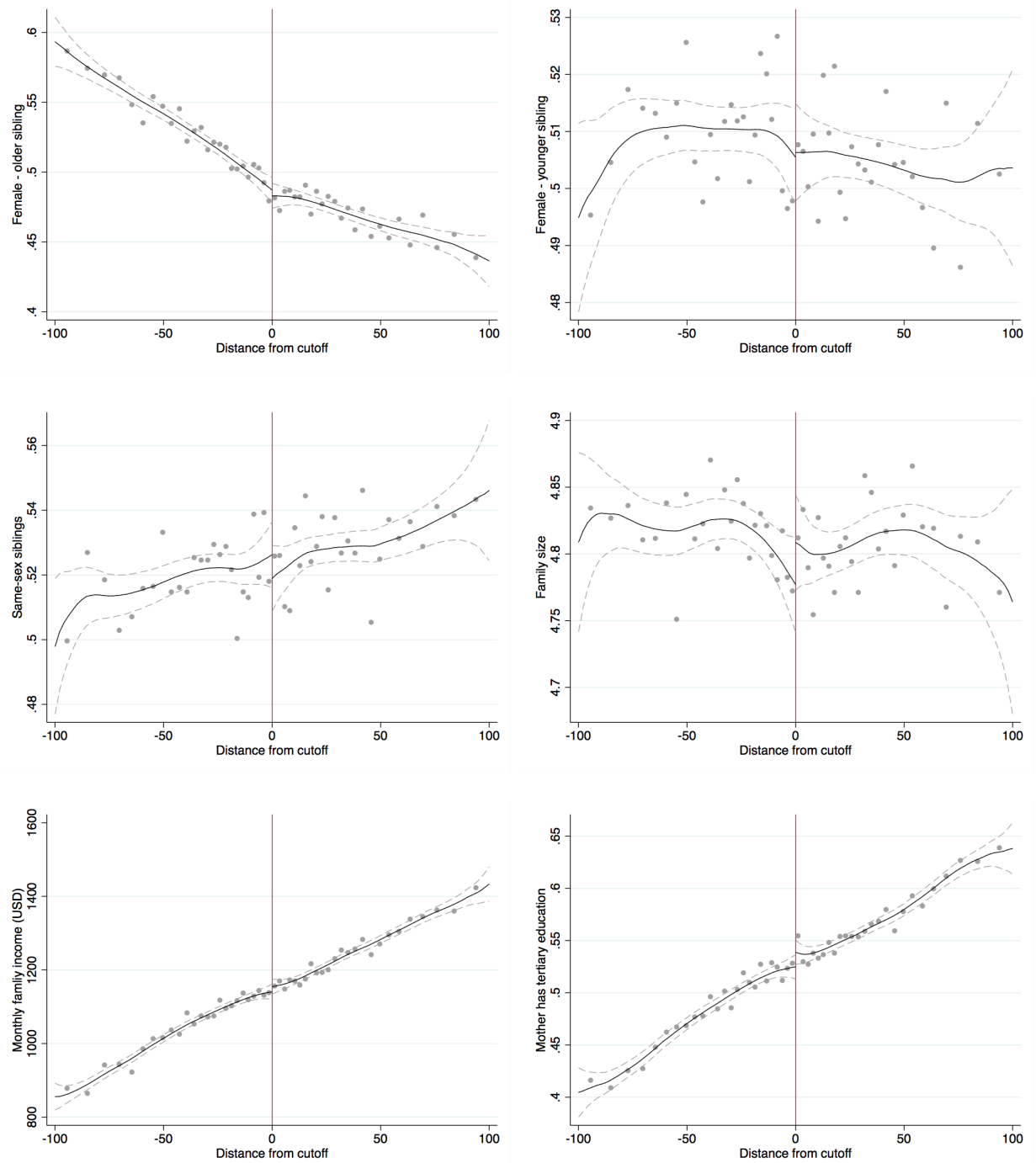
More importantly, we observe that younger siblings have an even stronger preference for attending the same major as their older sibling within their older siblings' college. In contrast to college spillovers, these effects are not restricted to siblings spending time together in college. Younger siblings are willing to follow their older siblings into their major/institution combinations, even when they are far apart in age and are unlikely to attend college simultaneously, and even in cases where they are likely to be well informed about the programs. Suggesting that younger siblings assign a symbolic or expressive value to attending the same major/institution as their older siblings.

While students can derive important non-pecuniary benefits from following their older siblings, this can have important implications for them. For example, having an older sibling enroll in a program that represents a good (or bad) match for the younger sibling could increase (or decrease) the younger siblings' graduation rates. Importantly, the fact that we are able to discard any positive effect of following one's sibling on graduation outcomes, highlights the potential for negative spillover effects on younger siblings' graduation outcomes.

Our findings raise the question about the extent to which higher education choices can be influenced by the choices of other members in a student's social network, such as high school friends, parents, or neighbors. A better understanding of these social influences, as well as of the structure of the social networks in which they operate, may be key to understanding phenomena such as social mobility and occupational segregation, and could help in the optimal design of policies aimed at improving students' higher education choices.

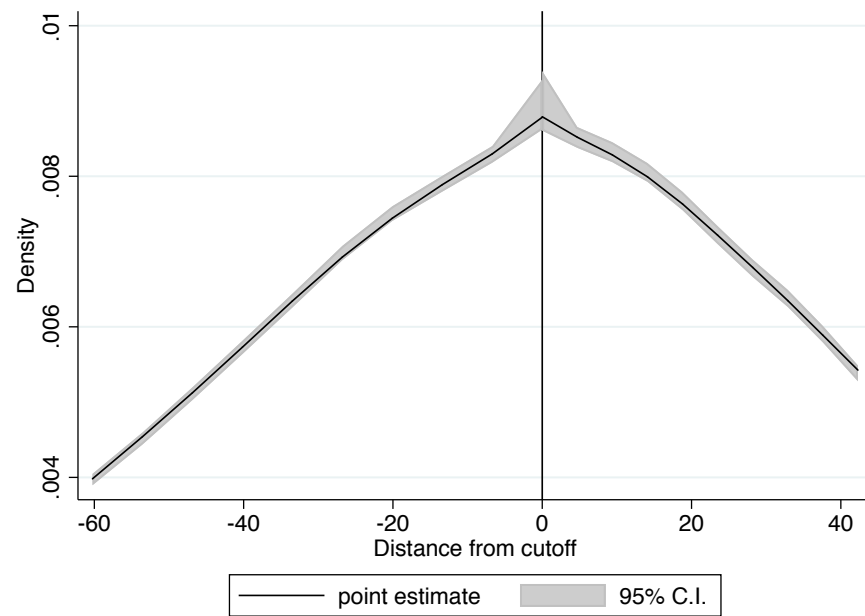
VIII Figures

Figure 3.1: Balance in Covariates



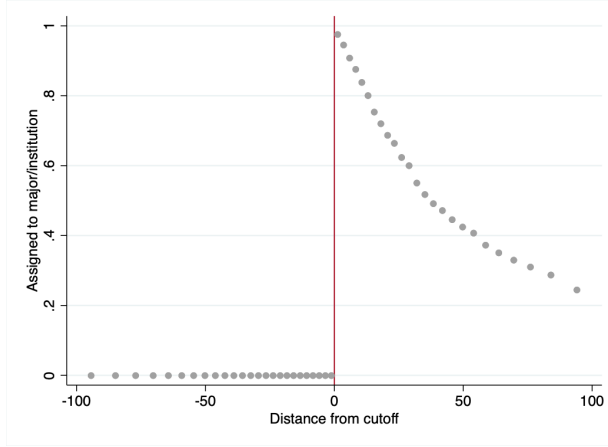
Notes: Each gray dot represents an equally sized group of observations (i.e., pivotal applications), with the group-mean of the standardized weighted score (i.e., \tilde{s}_{ijt}) in the horizontal axis, and the conditional group-mean of the respective covariate in the vertical axis. The red vertical line represents the point where $\tilde{s}_{ijt} = 0$. The gray continuous line shows local linear polynomial fits for the conditional mean of the covariate at both sides of the cutoff (triangular kernel with bandwidth $h = 50$), and the discontinuous lines show 95% confidence intervals for these conditional means.

Figure 3.2: Manipulation Check

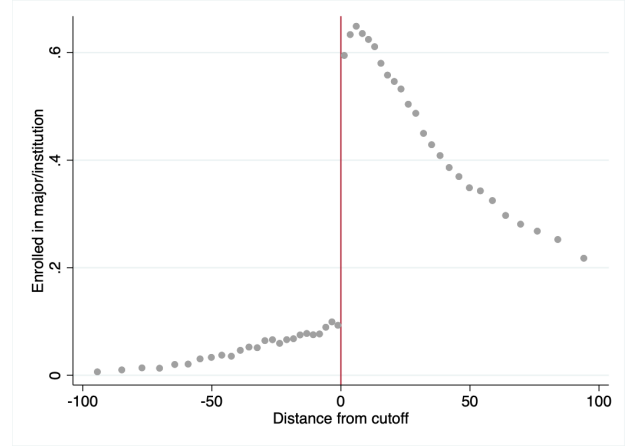


Notes: The figure shows the estimated density of \tilde{s}_{ijt} at both sides of the admission cutoff, with 95% confidence intervals estimated following [Cattaneo et al. \(2017\)](#). Sata command: `rddensity`.

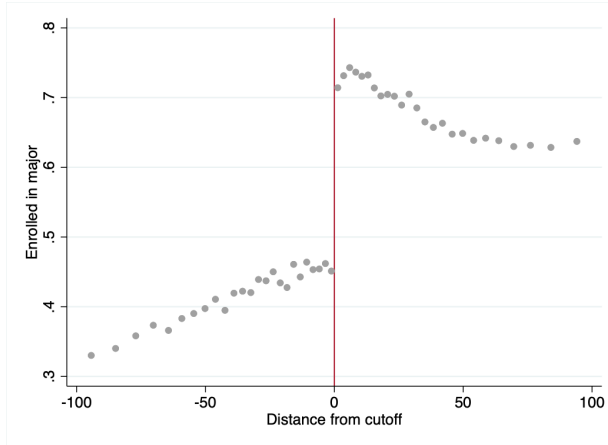
Figure 3.3: Discontinuity in Assignment and Enrollment



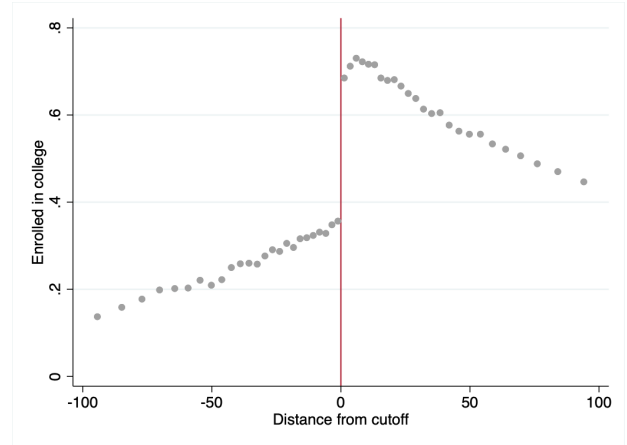
(a) Assignment to major/institution



(b) Enrollment in major/institution



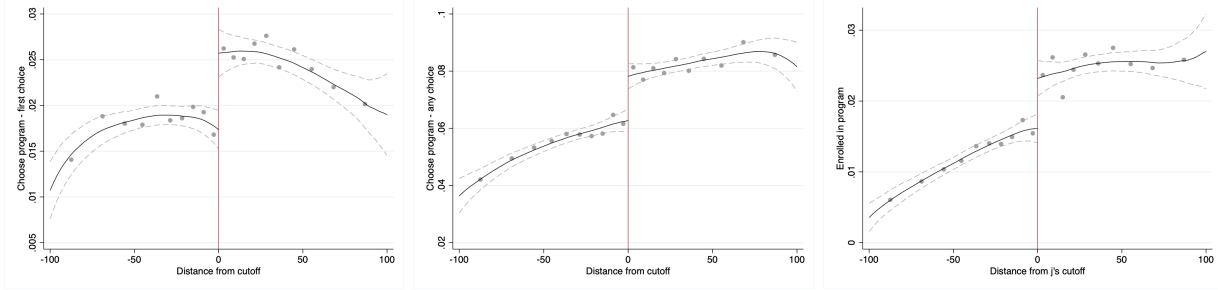
(c) Enrollment in major



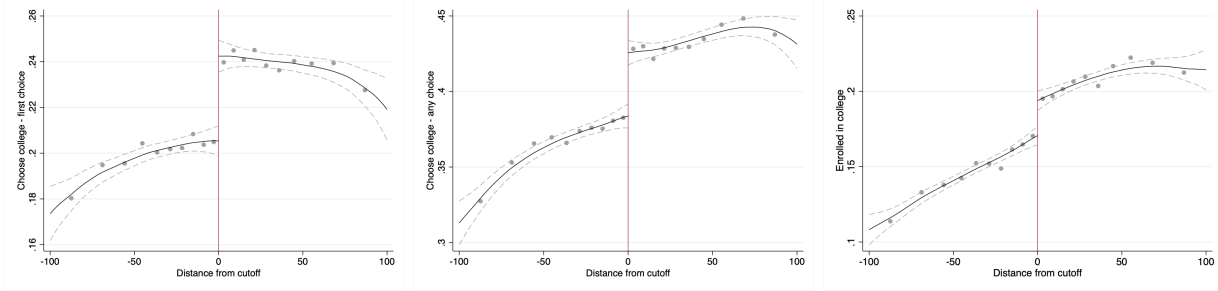
(d) Enrollment in college

Notes: Each gray dot represents an equally sized group of observations (i.e., pivotal applications), with the group-mean of the standardized weighted score (i.e., \tilde{s}_{ijt}) in the horizontal axis, and the conditional probability that the older sibling (a) is assigned to the cutoff major/institution, (b) enrolls in the cutoff major/institution, (c) enrolls in the cutoff major, or (d) enrolls in the cutoff college, in the vertical axis. The red vertical line represents the point where $\tilde{s}_{ijt} = 0$.

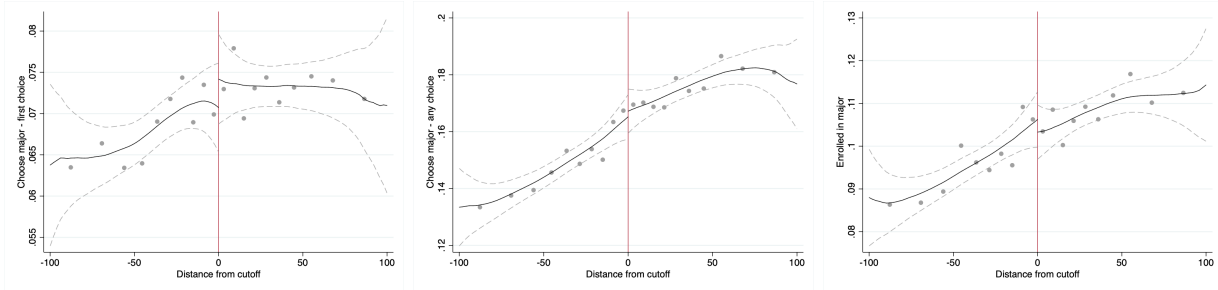
Figure 3.4: Sibling Spillovers in Choices



(a) Effects in choice of major/institution



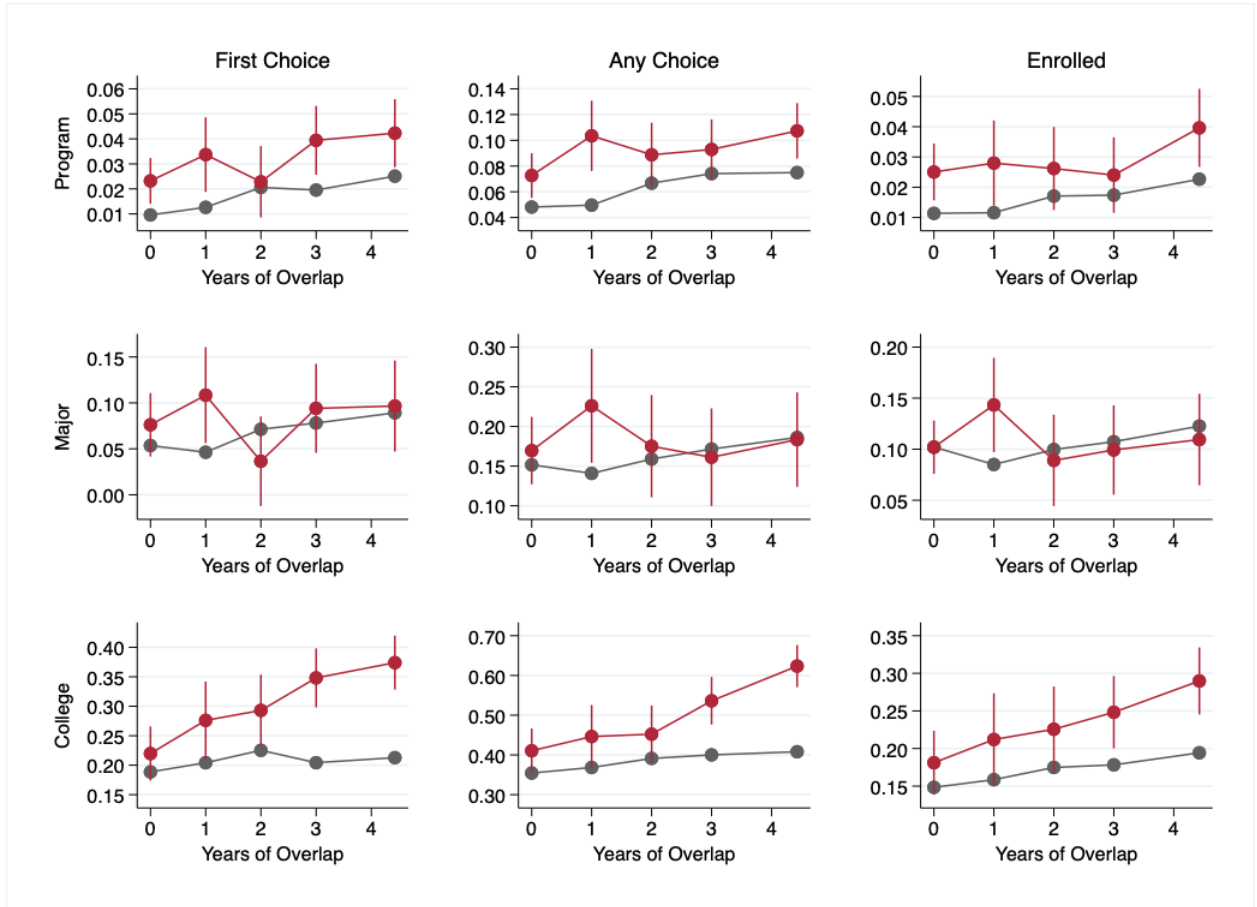
(b) Effects in choice of college



(c) Effects in choice of major

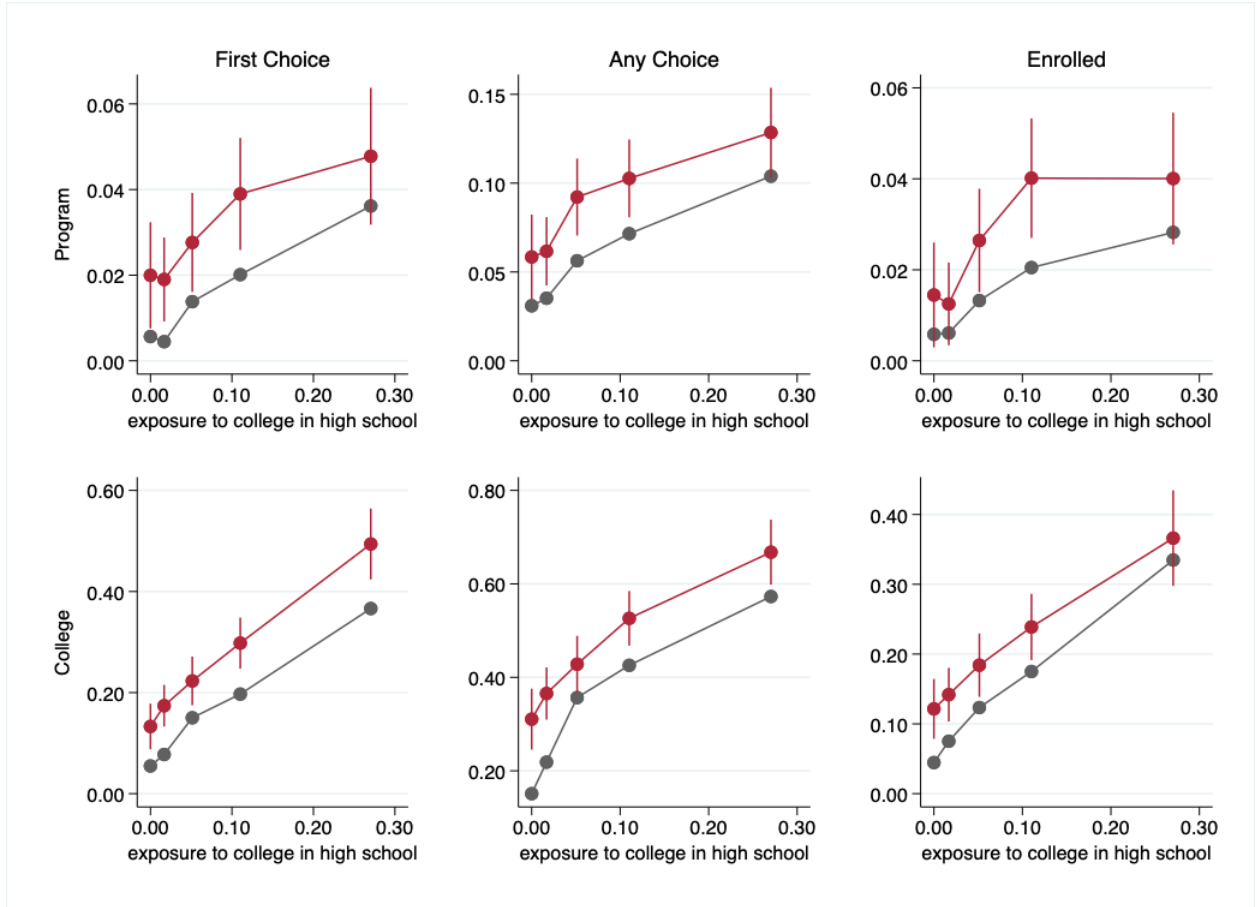
Notes: Each gray dot represents an equally sized group of observations (i.e., pivotal applications), with the group-mean of the standardized weighted score (i.e., \tilde{s}_{ijt}) in the horizontal axis, and the conditional probability that the younger sibling applies to or enrolls in (a) the cutoff major/institution, (b) the cutoff college, or (c) the cutoff major, in the vertical axis. The red vertical line represents the point where $\tilde{s}_{ijt} = 0$. The gray continuous line shows local linear polynomial fits for the conditional probability of the respective outcome at both sides of the cutoff (triangular kernel with bandwidth $h = 50$), and the discontinuous lines show 95% confidence intervals for these conditional probabilities.

Figure 3.5: Heterogeneous Effects by Years of Overlap



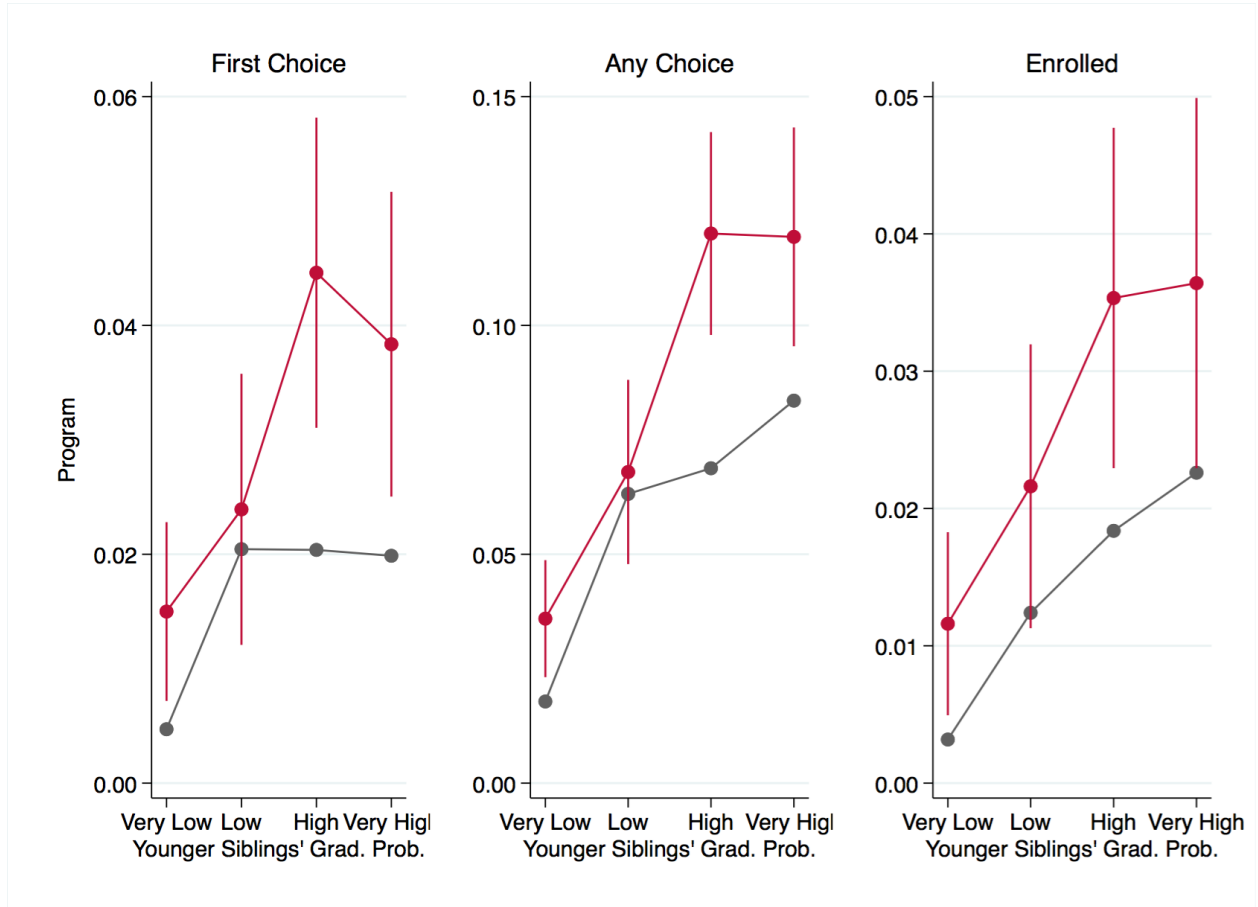
Notes: The plots show in the vertical axis graphical representations of δ_0^r (gray line) and β^r (red line) for $r = 1, \dots, 5$ in equation (3.7), where W_{ijt} equals the number of years siblings are expected to coincide in college. Vertical red lines show 95% confidence intervals for β^r coefficients. Interpretation of these plots is discussed in the results section.

Figure 3.6: Heterogeneous Effects by Previous Exposure to College



Notes: The plots show in the vertical axis graphical representations of δ_0^r (gray line) and β^r (red line) for $r = 1, \dots, 5$ in equation (??), where W_{ijt} equals percentage of students in the younger sibling's high school who enrolled in the cutoff college the year before the younger sibling applied. Vertical red lines show 95% confidence intervals for β^r coefficients. Interpretation of these plots is discussed in the results section.

Figure 3.7: Heterogeneous Effects by Match Quality



Notes: The plots show in the vertical axis graphical representations of δ_0^r (gray line) and β^r (red line) for $r = 1, \dots, 5$ in equation (??), where W_{ijt} equals the younger sibling's baseline probability of graduating from the pivotal major/institution. Vertical red lines show 95% confidence intervals for β^r coefficients. Interpretation of these plots is discussed in the results section.

IX Tables

Table 3.1: Descriptive Statistics

	All		Sample Older Siblings		Sample Cutoff	
	mean	s.d	mean	s.d	mean	s.d
<i>Socioeconomic Characteristics:</i>						
Female	0.52	0.50	0.52	0.50	0.49	0.50
Lives in the capital	0.41	0.49	0.42	0.49	0.38	0.49
Family size	4.35	1.88	4.88	1.76	4.81	1.76
Working family members	1.23	0.78	1.25	0.73	1.27	0.72
Father is household head	0.59	0.49	0.70	0.46	0.74	0.44
Mother is household head	0.30	0.46	0.25	0.43	0.23	0.42
Monthly family income (2017 USD)	856	903	1,067	1,100	1,529	1,265
Mother has primary education	0.22	0.42	0.17	0.38	0.06	0.25
Mother has secondary education	0.51	0.50	0.51	0.50	0.42	0.49
Mother has tertiary education	0.26	0.44	0.32	0.47	0.52	0.50
Father has primary education	0.22	0.41	0.17	0.38	0.07	0.25
Father has secondary education	0.47	0.50	0.46	0.50	0.36	0.48
Father has tertiary education	0.32	0.46	0.37	0.48	0.57	0.49
Father works full-time	0.64	0.48	0.71	0.45	0.77	0.42
Father works part-time	0.13	0.34	0.12	0.33	0.09	0.29
Mother works full-time	0.36	0.48	0.36	0.48	0.43	0.49
Mother works part-time	0.07	0.26	0.07	0.25	0.06	0.23
<i>Academic Performance:</i>						
GPA score	5.59	0.50	5.68	0.51	6.01	0.42
Math score	491	111	514	115	606	86
Language score	489	111	510	113	593	81
Observations	2,526,246		636,252		81,631	

Notes: For each variable, this table reports the mean and standard deviation. All includes all high school graduates who signed up for the standardized admission test in the 2004 to 2016 period. Sample older siblings includes all high school graduates for whom we identify a younger sibling. Sample cutoff considers only older siblings whose weighted score is within 50 points of the admission cutoff for at least one pivotal application.

Table 3.2: Balance Checks

Variable	N Obs	N Cluster	Mean C	T-C
Female - older sibling	121,482	81,199	0.487	0.001 (0.005)
Female - younger sibling	121,482	81,199	0.506	-0.001 (0.006)
Same-sex siblings	121,482	81,199	0.524	-0.003 (0.006)
Lives in the capital	121,482	81,199	0.399	0.006 (0.004)
Family size	121,482	81,199	4.786	0.019 (0.020)
Working family members	121,482	81,199	1.255	0.003 (0.008)
Father is household head	114,144	76,282	0.733	0.002 (0.005)
Mother is household head	114,144	76,282	0.232	-0.006 (0.005)
Family members in primary education	121,482	81,199	0.772	0.001 (0.009)
Family members in secondary education	121,482	81,199	1.417	0.014* (0.008)
Family members in tertiary education	121,482	81,199	0.316	0.000 (0.007)
Monthly family income (USD)	121,472	81,191	1,539	-12.478 (12.196)
Mother has primary education	113,616	75,871	0.062	-0.001 (0.003)
Mother has secondary education	113,616	75,871	0.409	0.004 (0.006)
Mother has tertiary education	113,616	75,871	0.529	-0.003 (0.005)
Father has primary education	111,196	74,279	0.064	0.002 (0.003)
Father has secondary education	111,196	74,279	0.349	0.002 (0.006)
Father has tertiary education	111,196	74,279	0.586	-0.003 (0.005)
Father works full-time	109,832	73,318	0.768	-0.001 (0.005)
Father works part-time	109,832	73,318	0.093	-0.003 (0.004)
Mother works full-time	113,565	75,813	0.430	0.002 (0.006)
Mother works part-time	113,565	75,813	0.057	-0.007** (0.003)

Notes: “Mean C” represents the mean of the covariate, conditional on the weighted score being just below the admission cutoff. This parameter corresponds to π_0 in a regression analogous to (3.5) where the covariate is used as the outcome, and where fixed effects are omitted. “T-C” represents the difference in means between observations just above the cutoff and those just below the cutoff. This parameter corresponds to τ in a regression analogous to (3.5) where the covariate is used as the outcome. Regressions are estimated by least squares, weighting observations using a triangular kernel centered at $\tilde{s}_{ijt} = 0$ and with bandwidth $h = 50$. Standard errors are shown in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.3: Younger Sibling's Higher Education Application

	Takes PSU:			Scores:				
	Math & Lang	History	Science	GPA	Language	Math	History	Science
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel B: Reduced Form</i>								
$\tilde{s}_{ij} \geq 0$	0.002 (0.002)	0.004 (0.006)	0.002 (0.005)	0.218 (1.121)	-0.844 (1.063)	-1.049 (1.096)	0.917 (1.462)	-1.267 (1.303)
Mean - C	0.975	0.568	0.678	583.624	561	578	552	558
N Obs	122,138	122,138	122,138	119,446	118,191	117,974	68,801	81,232
N Clusters	81,523	81,523	81,523	79,644	78,699	78,553	46,193	53,545

Notes: Table 3.3 shows for each outcome an estimate of τ in regression (3.5) which equals one if the older sibling crosses the threshold for a more preferred major/institution. "Mean C" represents the mean of the covariate, conditional on the weighted score being just below the admission cutoff. All regressions weight observations using a triangular kernel centered at $\tilde{s}_{ijt} = 0$ and with bandwidth $h = 50$. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.4: Sibling Spillovers in Choice of Major/Institution - RD Estimates

	Cutoff program listed as 1 st choice (1)	Cutoff program listed as any choice (2)	Enrolled in cutoff program (3)
<i>Panel A: First Stage</i>			
$\tilde{s}_{ij} \geq 0$	0.580*** (0.004)	0.580*** (0.004)	0.580*** (0.004)
<i>Panel B: Reduced Form</i>			
$\tilde{s}_{ij} \geq 0$	0.009*** (0.002)	0.017*** (0.003)	0.007*** (0.002)
<i>Panel C: IV Regressions</i>			
Enrolled in Program	0.015*** (0.003)	0.029*** (0.005)	0.013*** (0.003)
Mean - C	0.016	0.060	0.015
N Obs	122,138	122,138	122,138
N Clusters	81,523	81,523	81,523

Notes: Panel A shows the estimate of τ in a regression analogous to (3.5) where the outcome is an indicator for whether the older sibling ever enrolls in the cutoff major/institution. Panel B shows for each outcome an estimate of τ in regression (3.5). Panel C shows for each outcome the IV estimate of β in regression (3.6), where a cutoff-crossing indicator is used as instrument for the older sibling's enrollment in the cutoff major/institution. All regressions weight observations using a triangular kernel centered at $\tilde{s}_{ijt} = 0$ and with bandwidth $h = 50$. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.5: Sibling Spillovers in Choice of College - RD Estimates

	Full Sample			Next Best Alternative					
				Same College			Different College		
	(1) 1 st Choice	(2) Any Choice	(3) Enrolled	(4) 1 st Choice	(5) Any Choice	(6) Enrolled	(7) 1 st Choice	(8) Any Choice	(9) Enrolled
<i>Panel A: First Stage</i>									
<i>Enrolls in that Program</i>									
$\tilde{s}_{ij} \geq 0$	0.580*** (0.004)	0.580*** (0.004)	0.580*** (0.004)	0.587*** (0.008)	0.587*** (0.008)	0.587*** (0.008)	0.639*** (0.006)	0.639*** (0.006)	0.639*** (0.006)
<i>Enrolls in that College</i>									
$\tilde{s}_{ij} \geq 0$	0.388*** (0.005)	0.388*** (0.005)	0.388*** (0.005)	0.089*** (0.008)	0.089*** (0.008)	0.089*** (0.008)	0.605*** (0.007)	0.605*** (0.007)	0.605*** (0.007)
<i>Panel B: Reduced Form</i>									
$\tilde{s}_{ij} > 0$	0.038*** (0.005)	0.046*** (0.006)	0.024*** (0.004)	0.013 (0.009)	0.018* (0.010)	0.007 (0.009)	0.060*** (0.007)	0.067*** (0.008)	0.039*** (0.006)
<i>Panel C: 2SLS</i>									
Enrolled in Program	0.066*** (0.008)	0.078*** (0.010)	0.042*** (0.008)	0.022 (0.016)	0.030* (0.017)	0.012 (0.015)	0.094*** (0.010)	0.105*** (0.013)	0.061*** (0.010)
Mean - C	0.199	0.376	0.167	0.293	0.474	0.247	0.153	0.338	0.129
Enrolled in College	0.099*** (0.012)	0.117*** (0.014)	0.062*** (0.011)	0.145 (0.102)	0.198* (0.113)	0.076 (0.099)	0.099*** (0.011)	0.111*** (0.013)	0.064*** (0.010)
Mean - C	0.169	0.343	0.149	0.181	0.342	0.203	0.147	0.331	0.125
N Obs	122,138	122,138	122,138	37,606	37,606	37,606	57,554	57,554	57,554
N Cluster	81,523	81,523	81,523	31,464	31,464	31,464	44,247	44,247	44,247

Notes: Panel A shows estimates of major/institution and college compliance rates, corresponding to τ in a regression analogous to (3.5) where the outcome is an indicator for whether the older sibling ever enrolls in the cutoff major/institution or the cutoff college, respectively. Panel B shows for each outcome an estimate of τ in regressions analogous to (3.5), where the outcome is an indicator for whether the younger sibling chooses any major/institution in the cutoff college. Panel C shows for each outcome the IV estimate of β in a regression analogous to (3.6), where the outcome indicates whether the younger sibling chooses a major/institution in the cutoff college, and where the endogenous variable is an indicator for whether the older sibling ever enrolled in the cutoff major/institution or the cutoff college, respectively. All regressions weight observations using a triangular kernel centered at $\tilde{s}_{ijt} = 0$ and with bandwidth $h = 50$. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.6: Sibling Spillovers in Choice of Major - RD Estimates

	Full Sample			Next Best Alternative					
				Same Major			Different Major		
	(1) 1 st Choice	(2) Any Choice	(3) Enrolled	(4) 1 st Choice	(5) Any Choice	(6) Enrolled	(7) 1 st Choice	(8) Any Choice	(9) Enrolled
<i>Panel A: First Stage</i>									
<i>Enrolls in that Program</i>									
$\tilde{s}_{ij} \geq 0$	0.581*** (0.006)	0.581*** (0.006)	0.580*** (0.004)	0.654*** (0.011)	0.654*** (0.011)	0.655*** (0.011)	0.611*** (0.008)	0.611*** (0.008)	0.597*** (0.007)
<i>Enrolls in that Major</i>									
$\tilde{s}_{ij} \geq 0$	0.350*** (0.006)	0.350*** (0.006)	0.289*** (0.005)	0.039*** (0.010)	0.039*** (0.010)	0.041*** (0.009)	0.534*** (0.009)	0.534*** (0.009)	0.523*** (0.008)
<i>Panel B: Reduced Form</i>									
$\tilde{s}_{ij} > 0$	0.005 (0.004)	0.005 (0.005)	0.000 (0.002)	-0.004 (0.009)	0.002 (0.011)	0.001 (0.008)	0.003 (0.005)	0.010 (0.007)	-0.004 (0.004)
<i>Panel C: 2SLS</i>									
Enrolled in Program	0.008 (0.006)	0.009 (0.008)	0.000 (0.004)	-0.006 (0.013)	0.003 (0.017)	0.002 (0.013)	0.005 (0.008)	0.017 (0.011)	-0.007 (0.007)
Mean - C	0.071	0.165	0.376	0.084	0.141	0.081	0.048	0.115	0.052
Enrolled in Major	0.013 (0.011)	0.015 (0.013)	0.000 (0.007)	-0.104 (0.219)	0.054 (0.278)	0.029 (0.201)	0.006 (0.009)	0.019 (0.013)	-0.008 (0.008)
Mean - C	0.068	0.164	0.385	0.166	0.009	0.034	0.047	0.114	0.053
N Obs	74,646	74,646	122,138	17,774	17,774	19,455	35,346	35,346	40,816
N Cluster	50,864	50,864	81,523	14,993	14,993	16,499	28,245	28,245	32,558

Notes: Panel A shows estimates of degree and major compliance rates, corresponding to τ in a regression analogous to (3.5) where the outcome is an indicator for whether the older sibling ever enrolls in the cutoff major/institution or the cutoff major, respectively. Panel B shows for each outcome an estimate of τ in regressions analogous to (3.5), where the outcome is an indicator for whether the younger sibling chooses any major/institution in the cutoff major. Panel C shows for each outcome the IV estimate of β in a regression analogous to (3.6), where the outcome indicates whether the younger sibling chooses a major/institution in the cutoff major, and where the endogenous variable is an indicator for whether the older sibling ever enrolled in the cutoff major/institution or the cutoff major, respectively. All regressions weight observations using a triangular kernel centered at $\tilde{s}_{ijt} = 0$ and with bandwidth $h = 50$. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.7: Heterogeneous Spillovers by Overlap in College

	Choice of Program			Choice of College			Choice of Major		
	(1) 1 st choice	(2) any choice	(3) enrolled	(4) 1 st choice	(5) any choice	(6) enrolled	(7) 1 st choice	(8) any choice	(9) enrolled
<i>0 years overlap</i>									
Enrolled	0.014*** (0.005)	0.026*** (0.009)	0.012** (0.005)	0.031 (0.024)	0.056* (0.029)	0.035 (0.022)	0.021 (0.018)	0.010 (0.022)	−0.011 (0.013)
Mean - C	0.009	0.046	0.010	0.175	0.333	0.137	0.048	0.149	0.105
N Obs	32,484	32,484	32,484	32,484	32,484	32,484	14,751	14,751	14,751
N Clusters	24,058	24,058	24,058	24,058	24,058	24,058	11,618	11,618	11,618
<i>1 to 2 years overlap</i>									
Enrolled	0.013** (0.006)	0.037*** (0.010)	0.014*** (0.005)	0.060*** (0.023)	0.055** (0.027)	0.048** (0.021)	0.009 (0.019)	0.046* (0.025)	0.021 (0.017)
Mean - C	0.016	0.056	0.013	0.190	0.356	0.148	0.060	0.138	0.092
N Obs	32,484	32,484	32,484	32,484	32,484	32,484	14,751	14,751	14,751
N Clusters	24,058	24,058	24,058	24,058	24,058	24,058	11,618	11,618	11,618
<i>3 or more years overlap</i>									
Enrolled	0.019*** (0.005)	0.024*** (0.008)	0.012** (0.005)	0.150*** (0.017)	0.176*** (0.020)	0.081*** (0.017)	0.013 (0.019)	−0.008 (0.023)	−0.013 (0.017)
Mean - C	0.021	0.072	0.019	0.155	0.345	0.158	0.083	0.189	0.125
N Obs	32,484	32,484	32,484	32,484	32,484	32,484	14,751	14,751	14,751
N Clusters	24,058	24,058	24,058	24,058	24,058	24,058	11,618	11,618	11,618

Notes: Reported coefficients correspond to IV estimates of regression (3.7), where the outcomes are indicators for whether the younger sibling applies to or enrolls in the cutoff degree (columns 1-3), the cutoff college (columns 4-6) or the cutoff major (columns 7-9); the endogenous variables are indicators for whether the older sibling ever enrolled in the cutoff degree (columns 1-3), the cutoff college (columns 4-6) or the cutoff major (columns 7-9), as well as these indicators interacted with an indicator for whether siblings are expected to coincide in college (W_{ijt} in the model). Endogenous variables are instrumented by cutoff-crossing indicators and cutoff-crossing indicators interacted with W_{ijt} . All regressions weight observations using a triangular kernel centered at $\tilde{s}_{ijt} = 0$ and with bandwidth $h = 50$. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.8: Heterogeneous Spillovers by Sex

	Choice of Program			Choice of College			Choice of Major		
	(1) 1 st choice	(2) any choice	(3) enrolled	(4) 1 st choice	(5) any choice	(6) enrolled	(7) 1 st choice	(8) any choice	(9) enrolled
<i>Panel A: Full Sample</i>									
Enrolled	0.010*** (0.004)	0.035*** (0.007)	0.013*** (0.004)	0.064*** (0.011)	0.087*** (0.014)	0.040*** (0.011)	0.005 (0.009)	0.026** (0.011)	0.000 (0.008)
Enrolled × Same-sex	0.009 (0.006)	−0.011 (0.010)	−0.000 (0.006)	0.005 (0.016)	−0.016 (0.019)	0.004 (0.015)	0.005 (0.013)	−0.032** (0.016)	0.001 (0.011)
N Obs	122,138	122,138	122,138	122,138	122,138	122,138	74,646	74,646	74,646
N Clusters	81,523	81,523	81,523	81,523	81,523	81,523	50,864	50,864	50,864
<i>Panel B: Younger Sisters</i>									
Enrolled	0.004 (0.005)	0.028*** (0.009)	0.009* (0.005)	0.069*** (0.015)	0.091*** (0.018)	0.030** (0.014)	−0.005 (0.012)	0.021 (0.013)	−0.001 (0.010)
Enrolled × Same-sex	0.013* (0.008)	−0.009 (0.014)	0.003 (0.007)	0.015 (0.023)	−0.001 (0.027)	0.019 (0.021)	0.028 (0.017)	−0.024 (0.021)	0.014 (0.015)
N Obs	61,810	61,810	61,810	61,810	61,810	61,810	37,700	37,700	37,700
N Clusters	41,411	41,411	41,411	41,411	41,411	41,411	25,736	25,736	25,736
<i>Panel C: Younger Brothers</i>									
Enrolled	0.017*** (0.006)	0.043*** (0.010)	0.019*** (0.006)	0.065*** (0.017)	0.084*** (0.021)	0.048*** (0.017)	0.017 (0.014)	0.032* (0.017)	0.004 (0.012)
Enrolled × Same-sex	0.006 (0.009)	−0.014 (0.015)	−0.006 (0.009)	−0.005 (0.023)	−0.023 (0.028)	−0.008 (0.022)	−0.013 (0.019)	−0.038 (0.024)	−0.010 (0.017)
N Obs	59,981	59,981	59,981	59,981	59,981	59,981	36,605	36,605	36,605
N Clusters	39,906	39,906	39,906	39,906	39,906	39,906	24,925	24,925	24,925

Notes: Panel A shows IV estimates of regression (3.7), where the outcomes are indicators for whether the younger sibling applies to or enrolls in the cutoff degree (columns 1-3), the cutoff college (columns 4-6) or the cutoff major (columns 7-9); the endogenous variables are indicators for whether the older sibling ever enrolled in the cutoff degree (columns 1-3), the cutoff college (columns 4-6) or the cutoff major (columns 7-9), as well as these indicators interacted with an indicator of same-sex sibling pairs (W_{ijt} in the model). Endogenous variables are instrumented by cutoff-crossing indicators and cutoff-crossing indicators interacted with W_{ijt} . Panels B and C show analogous results for the sub-samples where the younger siblings are females and males, respectively. All regressions weight observations using a triangular kernel centered at $\bar{s}_{ijt} = 0$ and with bandwidth $h = 50$. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.9: Sibling Spillovers on Younger Sibling's Graduation Outcomes

	Younger sibling enrolls in Older sibling's:		
	Program (1)	College (2)	Major (3)
Enrolled	0.016 (0.096)	-0.016 (0.027)	0.051 (0.095)
Mean - C	0.830	0.733	0.592
Obs	1, 332	14, 148	2, 301

Notes: Table 3.9 presents the IV estimate of β in regression (3.6), where a cutoff-crossing indicator is used as instrument for the younger sibling's enrollment in a cutoff major/institution where the older sibling enrolled, and the outcomes equals one of the younger sibling ever graduated. All regressions weight observations using a triangular kernel centered at $\tilde{s}_{ijt} = 0$ and with bandwidth $h = 50$. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.10: Placebo test: Spillovers from Younger to Older Siblings

	Choice of Program			Choice of College			Choice of Major		
	(1) 1 st choice	(2) any choice	(3) enrolled	(4) 1 st choice	(5) any choice	(6) enrolled	(7) 1 st choice	(8) any choice	(9) enrolled
<i>Panel A: First Stage</i>									
<i>Enrolls in that Program</i>	0.570*** (0.005)	0.570*** (0.005)	0.570*** (0.005)	0.570*** (0.005)	0.570*** (0.005)	0.570*** (0.005)	0.574*** (0.005)	0.574*** (0.005)	0.574*** (0.005)
<i>Enrolls in that College</i>	0.383*** (0.005)	0.383*** (0.005)	0.383*** (0.005)	0.383*** (0.005)	0.383*** (0.005)	0.383*** (0.005)	0.387*** (0.006)	0.387*** (0.006)	0.387*** (0.006)
<i>Enrolls in that Major</i>	0.310*** (0.005)	0.310*** (0.005)	0.310*** (0.005)	0.310*** (0.005)	0.310*** (0.005)	0.310*** (0.005)	0.332*** (0.005)	0.332*** (0.005)	0.332*** (0.005)
<i>Panel B: Reduced Form</i>									
$\tilde{s}_{ij} \geq 0$	0.000 (0.002)	-0.003 (0.003)	-0.001 (0.002)	-0.001 (0.005)	-0.005 (0.005)	0.007 (0.005)	-0.001 (0.003)	-0.006 (0.004)	-0.006** (0.003)
Mean - C	0.022	0.073	0.031	0.216	0.402	0.213	0.064	0.158	0.115
<i>Panel C: IV Regressions</i>									
Enrolled in Program	0.000 (0.003)	-0.005 (0.005)	-0.002 (0.004)						
Enrolled in Major				-0.004 (0.012)	-0.013 (0.014)	0.017 (0.013)			
Enrolled in College							-0.003 (0.009)	-0.017 (0.012)	-0.019** (0.010)
N Obs	114,737	114,737	114,737	114,737	114,737	114,737	102,166	102,166	102,166
N Clusters	78,940	78,940	78,940	78,940	78,940	78,940	70,474	70,474	70,474

Notes: Panel A shows estimates of degree, college and major compliance rates of the younger siblings, corresponding to τ in a regression analogous to (3.5) where the outcome is an indicator for whether the younger sibling ever enrolls in the cutoff degree, college or major, depending on the case. Panel B shows for each outcome an estimate of τ in regressions analogous to (3.5), where the outcome is an indicator for whether the older sibling chooses the cutoff degree (columns 1-3), the cutoff college (columns 4-6) or the cutoff major (columns 7-9). Panel C shows for each outcome the IV estimate of β in a regression analogous to (3.6), where the outcome indicates whether the older sibling chooses the cutoff degree (columns 1-3), the cutoff college (columns 4-6) or the cutoff major (columns 7-9), and where the endogenous variable is an indicator for whether the younger sibling ever enrolled in the cutoff degree (columns 1-3), cutoff college (columns 4-6), or cutoff major (columns 7-9). All regressions weight observations using a triangular kernel centered at $\tilde{s}_{ijt} = 0$ and with bandwidth $h = 50$. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Appendices

1.A Test Scores Before and After Targeted Vouchers

This section reviews how test scores evolved during the 2005-2012 period for eligible versus ineligible students. Estimates indicate that by 2012, the gap in test scores between these two groups had decreased by roughly 0.08 standard deviations. Results can be found in Figure 1.A.1 and Table 1.A.1. Previous studies that have looked at this result include Neilson (2013) and Feigenberg et al. (2017). In both cases, the authors have to make some assumption about which students would have been eligible in the past, because data on eligibility is only available as of 2008. Given that I have data on the socioeconomic ranking for the whole population of students in 2012, I can easily construct a measure of eligibility for previous cohorts by characterizing as eligible those students who in 2012 were below the threshold for eligibility. Estimates using this improved measure of eligibility for previous cohorts indicate that the difference in test scores between eligible and ineligible students decreased in this period, but that changes are below those reported by previous studies that are of the order of 0.2 standard deviations.

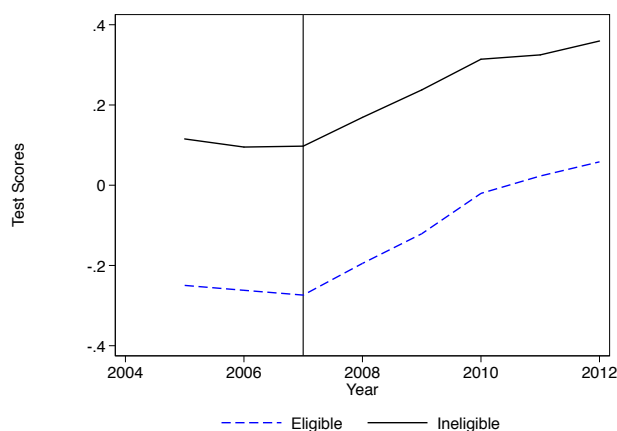


Figure 1.A.1: Test Scores Before and After Targeted Vouchers

Test scores equal the students' performance on standardized Math and Language tests that are applied nationwide to students in 4th grade.

Table 1.A.1: Test Scores Before and After Targeted Vouchers

	(1) Test Scores
Student Eligible	-0.104 (0.00612)
Student Eligible x 2006	0.0311 (0.00743)
Student Eligible x 2007	0.0189 (0.00788)
Student Eligible x 2008	0.0306 (0.00761)
Student Eligible x 2009	0.0208 (0.00797)
Student Eligible x 2010	0.0548 (0.00767)
Student Eligible x 2011	0.0932 (0.00772)
Student Eligible x 2012	0.0843 (0.00794)

Estimates are at the student level. Test scores equal the students' performance on standardized math and language tests that are applied nationwide to students in 4th grade. Estimates include year fixed effects and controls for mother's education, father's education and income. Standard errors are clustered at the school level.

1.B School Characteristics in 2007

This section presents average school characteristics in 2007 for schools that in 2012: where public; where private voucher and not charging add-ons to parents; where private voucher, charging add-ons to parents, and had joined the policy; where private voucher, charging add-ons to parents, and had not joined the policy.

Table 1.B.1: Schools' Characteristics in 2007

	(1) Test Scores	(2) Test Scores Language	(3) Test Scores Math	(4) SES	(5) Add-on	(6) Size	(7) Class Size
Public (57%)	235.8 (27.5)	242.6 (27.5)	228.5 (30.5)	9.4 (1.8)	0.0 (0.0)	26.5 (30.0)	17.4 (13.3)
Private Voucher w/No Add-On that joined the policy (19%)	231.7 (29.0)	240.4 (28.7)	222.7 (32.1)	9.4 (2.2)	0.0 (0.0)	25.9 (29.8)	18.3 (14.6)
Private Voucher w/Add-On that joined the policy (13%)	256.0 (22.9)	259.5 (22.2)	252.1 (24.7)	12.3 (1.2)	41.8 (34.2)	54.5 (37.9)	32.7 (9.9)
Private Voucher w/Add-On that didn't join the policy (11%)	264.3 (22.7)	267.3 (21.8)	260.9 (24.7)	13.1 (1.2)	69.4 (45.9)	57.2 (43.0)	31.0 (9.3)

Includes all subsidized primary schools in 2012. Test score equals the average result of the schools on the 4th grade standardized test in 2007, SES equals the average years of education of mothers' of students attending those school, add-on equal the total amount charged to non-eligible parents in those school, school size equals the cohort size at those school, class size equals the average class size at those school.

1.C Estimates with Alternative Bandwidths

This section presents the main results from this study using alternative bandwidths. Results from Table 1.4 use optimal bandwidths computed using [Calonico et al. \(2014\)](#). These optimal bandwidths range between 800 to 1500 points. In this section I present estimates using a 500, 1000, 1500 and 2000 bandwidths. All estimates are for the effect of being eligible for a targeted voucher, where the discontinuity is used as an instrument for eligibility.

Table 1.C.1: School Choice and Educational Outcomes with Alternative Bandwidths

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Panel A: 500 Bandwidth									
Eligible	0.0299 (0.0263)	0.0476 (0.0387)	0.0462 (0.0337)	4.037 (1.680)	0.165 (0.551)	0.0141 (0.143)	-0.0280 (0.0606)	-0.0251 (0.0557)	-0.107 (0.0512)
Mean	0.616	0.666	1.019	38.558	30.615	1.313	-0.097	0.236	0.273
Obs left	6569	6370	6358	6540	6467	2489	5114	4815	4845
Obs right	6879	6669	6659	6846	6770	2627	5421	5125	5148
Panel A: 1000 Bandwidth									
Eligible	0.00921 (0.0184)	0.0148 (0.0270)	0.0338 (0.0234)	3.092 (1.175)	0.0892 (0.387)	0.0540 (0.0969)	-0.0366 (0.0421)	-0.0106 (0.0392)	-0.0526 (0.0361)
Mean	0.616	0.666	1.019	38.558	30.615	1.313	-0.097	0.236	0.273
Obs left	12865	12464	12439	12811	12653	4761	10034	9493	9525
Obs right	13470	13056	13040	13408	13258	5174	10640	10125	10178
Panel C: 1500 Bandwidth									
Eligible	0.00125 (0.0151)	0.00161 (0.0221)	0.0223 (0.0191)	2.903 (0.968)	-0.219 (0.318)	0.0487 (0.0785)	-0.0365 (0.0346)	-0.0251 (0.0324)	-0.0403 (0.0298)
Mean	0.616	0.666	1.019	38.558	30.615	1.313	-0.097	0.236	0.273
Obs left	19126	18528	18497	19043	18821	7046	14871	14094	14110
Obs right	20640	19991	19965	20542	20314	7882	16370	15580	15640
Panel D: 2000 Bandwidth									
RD.Estimate	-0.00260 (0.0132)	-0.00294 (0.0193)	0.0140 (0.0166)	2.698 (0.844)	-0.318 (0.277)	0.0365 (0.0670)	-0.0225 (0.0302)	-0.0203 (0.0284)	-0.0332 (0.0261)
Mean	0.616	0.666	1.019	38.558	30.615	1.313	-0.097	0.236	0.273
Obs left	27274	26394	26350	27158	26816	10917	21142	20027	20031
Obs right	27390	26519	26480	27261	26950	11159	21795	20800	20853

Results from `rdrobust` ([Calonico et al., 2014](#)). All estimates include controls for mother's education, father's education and region. All cells contains instrumental variable estimates, where the discontinuity is used as an instrument for being eligible for a targeted voucher in 2012.

1.D Robustness Checks for Heterogeneous Results

Table 1.D.1 : Robustness Check: Heterogeneous Effects by Mothers' Education

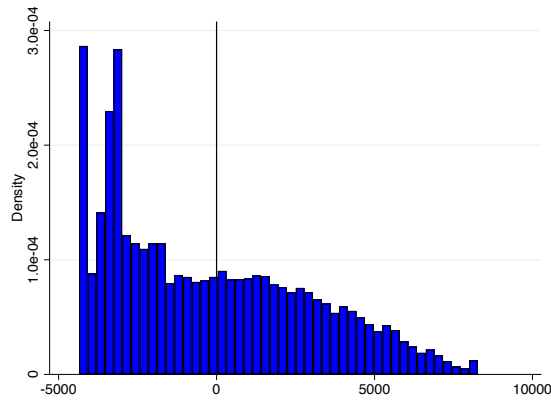
	(1) Mother's Education	(2) Father's Education	(3) Income	(4) Books	(5) Internet	(6) Computer	(7) Attended Childcare (0-2)
Mother has less than High School Education							
$R \leq Cutoff$	-0.0249 (0.0960)	0.00177 (0.115)	-0.192 (13.23)	1.326 (0.922)	0.00904 (0.0242)	-0.0164 (0.0241)	0.0488 (0.0198)
Mean Control	7.877	8.758	421.1	17.14	0.326	0.524	0.157
Observations	10,217	11,702	9,267	7,995	7,110	7,927	7,037
Mother has High School Education or Tertiary Education							
$R \leq Cutoff$	-0.0471 (0.0406)	0.0279 (0.0901)	-28.41 (21.99)	-0.510 (0.963)	-0.00464 (0.0174)	0.00976 (0.0137)	0.00802 (0.0145)
Mean Control	12.66	11.53	753.0	29.63	0.599	0.791	0.224
Observations	13,671	14,824	13,773	13,653	15,154	16,708	15,870

Results from `rdrobust` (Calonico et al., 2014). Mother's education and father's education equal total years of education. Books equal the total amount of books in the house and Internet and Computer are dummy variables that equal one if the family has internet and/or a computer. Attended Childcare is a dummy variable that equals one if the child attended childcare when 0-2 years old and when 3-4 years old. Standard errors in parenthesis.

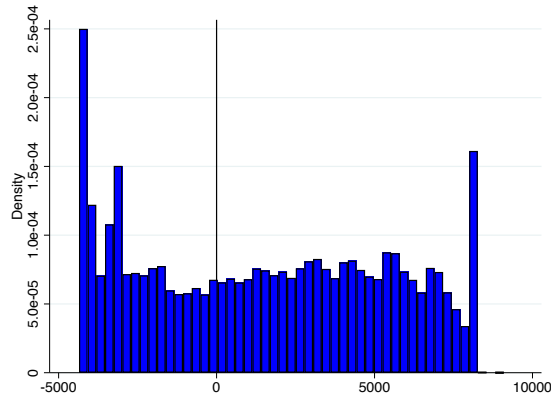
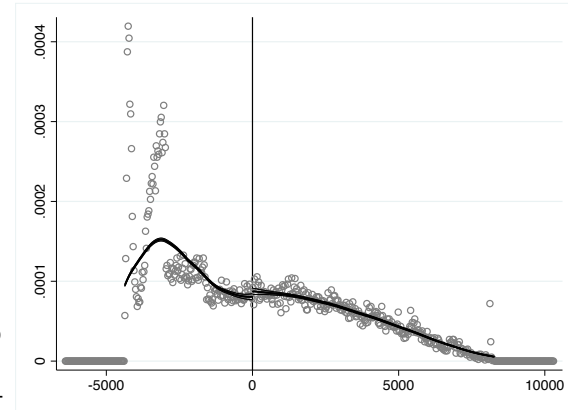
Table 1.D.2: Robustness Check: Heterogeneous Effects by Distance to Nearest Private Voucher School with Add-ons that Joined the Policy

	(1) Mother's Education	(2) Father's Education	(3) Income	(4) Books	(5) Internet	(6) Computer	(7) Attended Childcare (0-2)
Nearest P. Voucher School with Add-ons is less than 0.4 miles away							
$R \leq Cutoff$	0.0993 (0.165)	0.0664 (0.170)	-14.74 (27.43)	-0.122 (1.519)	0.0287 (0.0276)	0.0192 (0.0207)	-0.00345 (0.0234)
Mean Control	11.01	10.72	682.3	27.70	0.572	0.728	0.221
Observations	4,766	5,717	7,347	5,268	6,060	8,817	5,674
Nearest P. Voucher School with Add-ons is more than 0.4 miles away							
$R \leq Cutoff$	-0.0971 (0.165)	-0.136 (0.189)	-22.45 (35.21)	-0.666 (1.444)	-0.0341 (0.0319)	-0.0420 (0.0283)	0.0219 (0.0238)
Mean Control	10.78	10.59	668.9	27.02	0.575	0.738	0.215
Observations	5,787	4,560	4,738	5,671	4,421	4,700	5,692

Note: Results from `rdrobust` (Calonico et al., 2014). Mother's education and father's education equal total years of education. Books equal the total amount of books in the house and Internet and Computer are dummy variables that equal one if the family has internet and/or a computer. Attended Childcare is a dummy variable that equals one if the child attended childcare when 0-2 years old and when 3-4 years old. Standard errors in parenthesis.



(a) Mother has less than High School Education



(b) Mother has High School Education or Tertiary Education

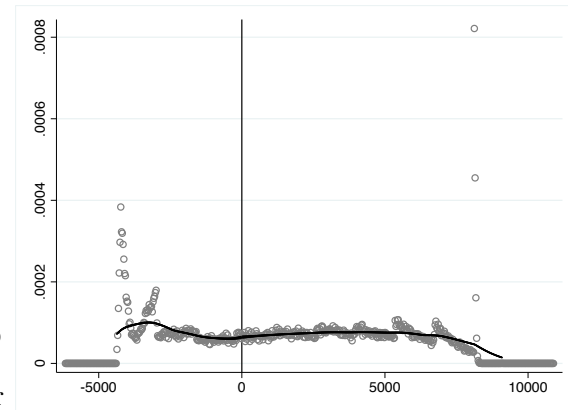
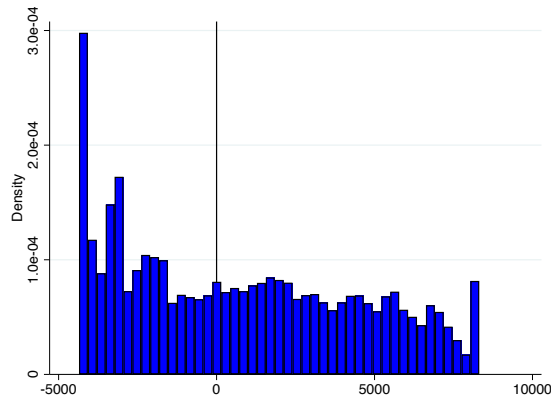
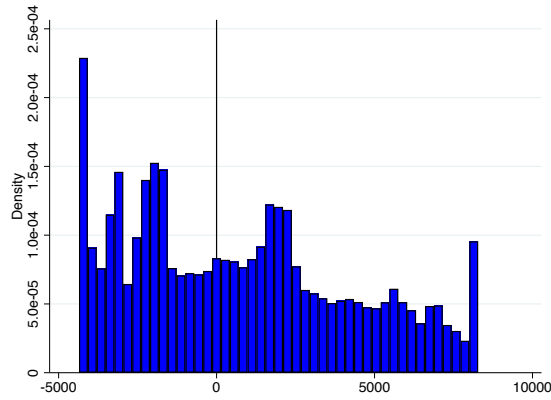
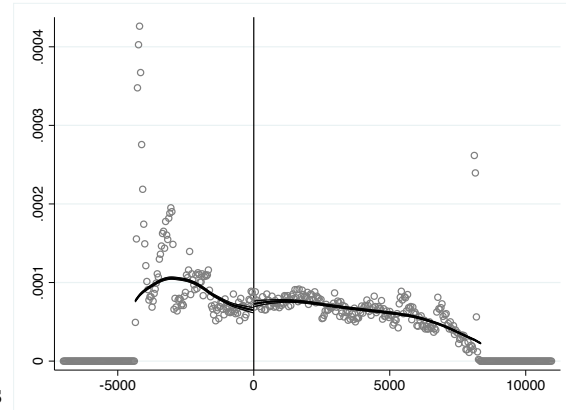


Figure 1.D.1: Visual Evaluation of Robustness Checks:Heterogeneous Effects by Mothers' Education



(a) Nearest P. Voucher School with Add-ons is less than 0.4 miles away



(b) Nearest P. Voucher School with Add-ons is more than 0.4 miles away

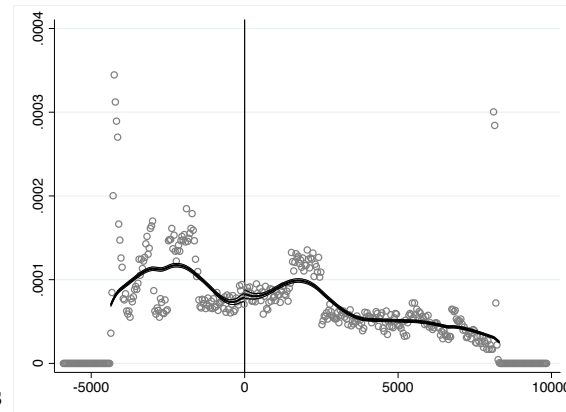


Figure 1.D.2: Visual Evaluation of Robustness Checks: Heterogeneous Effects by Distance to Nearest Private Voucher School with Add-ons that Joined the Policy

2.A Estimates Using Alternative Specifications

Table 2.A.1: Estimates using Alternative Bandwidths

	Loans TS vs No Loans				Loans TS & U vs Loans TS				Loans TS & U vs No Loans			
	BW=0.5		BW=0.7		BW=55		BW=75		BW=60		BW=80	
	Mean Control	Est	Mean Control	Est	Mean Control	Est	Mean Control	Est	Mean Control	Est	Mean Control	Est
Enrollment												
Technical Degree	0.489	-0.006 (0.007)	0.490	-0.008 (0.006)	0.355	-0.073*** (0.006)	0.362	-0.084*** (0.005)	0.366	-0.055*** (0.010)	0.377	-0.065*** (0.009)
Professional Degree TS	0.149	0.043*** (0.005)	0.147	0.043*** (0.005)	0.195	-0.059*** (0.005)	0.197	-0.062*** (0.004)	0.177	0.007 (0.008)	0.180	0.007 (0.007)
Professional Degree Univ	0.233	-0.021*** (0.006)	0.236	-0.023*** (0.005)	0.400	0.137*** (0.006)	0.392	0.152*** (0.005)	0.396	0.060*** (0.010)	0.382	0.073*** (0.009)
Any Degree	0.870	0.016*** (0.005)	0.873	0.012*** (0.004)	0.950	0.005 (0.003)	0.950	0.006** (0.002)	0.939	0.013** (0.006)	0.938	0.015*** (0.005)
Graduation												
Technical Degree	0.280	-0.004 (0.006)	0.277	0.000 (0.005)	0.255	-0.034*** (0.005)	0.259	-0.039*** (0.005)	0.217	-0.031*** (0.009)	0.223	-0.037*** (0.008)
Professional Degree TS	0.083	0.019*** (0.004)	0.081	0.021*** (0.004)	0.143	-0.037*** (0.004)	0.146	-0.039*** (0.004)	0.092	-0.002 (0.006)	0.093	0.000 (0.005)
Professional Degree Univ	0.099	-0.002 (0.004)	0.100	-0.002 (0.004)	0.220	0.056*** (0.006)	0.216	0.063*** (0.005)	0.155	0.002 (0.008)	0.150	0.010 (0.007)
Any Degree	0.439	0.010 (0.007)	0.434	0.017*** (0.006)	0.580	-0.007 (0.007)	0.582	-0.006 (0.006)	0.441	-0.028*** (0.011)	0.442	-0.024** (0.009)
Actual Costs (At Graduation)												
Total Years	3.543	0.150*** (0.029)	3.547	0.144*** (0.025)	4.505	0.162*** (0.026)	4.490	0.202*** (0.022)	4.271	0.239*** (0.045)	4.234	0.274*** (0.040)
Total Cost (2017 USD)	6,940	368.499*** (93.959)	6,964	362.917*** (80.880)	10,519	973.294*** (122.311)	10,408	1,136.293*** (107.643)	9,944	832.115*** (184.040)	9,805	988.746*** (159.747)
Expected Benefits (At Graduation)												
Expected Income	10,618	-5.437 (49.023)	10,611	19.832 (42.377)	12,065	126.514* (65.485)	12,038	127.747** (58.370)	11,130	-152.924* (85.750)	11,085	-97.275 (75.820)
Expected Employment	0.592	-0.000 (0.001)	0.592	-0.000 (0.001)	0.628	0.005*** (0.002)	0.627	0.005*** (0.001)	0.607	-0.005** (0.002)	0.607	-0.004** (0.002)
Benefits-Costs												
PV Benefits-Costs	-340	-1,877.235* (1,010.120)	-563	-1,257.917 (872.589)	18,415	297.222 (1,379.901)	18,063	-199.571 (1,233.601)	1,143	-6,265.543*** (1,803.684)	617	-5,489.445*** (1,593.374)
Obs	86,978		103,630		87,266		114,562		31,203		39,338	

Table 2.A.1 shows RD estimates using alternative bandwidths. Actual Costs (At Graduation) refer to how much students have spent 7 to 9 years after high school graduation. Expected Benefits (At Graduation) refer to expected annual incomes four years after graduation and employment probabilities one year after graduation based on where students have graduated from. I assume an expected annual wage of 8,844 USD and an expected employment rate of 0.55 for students who do not graduate or never enroll in higher education. PV Benefits-Costs are present-discounted expected earnings net of costs, assuming an interest rate of 3.5% and zero wage growth (see Section D for details). *** p < 0.01, ** p < 0.05, * p < 0.10.

Table 2.A.2: Estimates using Polynomials of the Running Variable

	Loans TS vs No Loans				Loans TS & U vs Loans TS				Loans TS & U vs No Loans			
	Poly.=2		Poly.=3		Poly.=2		Poly.=3		Poly.=2		Poly.=2	
	Mean	Est	Mean	Est	Mean	Est	Mean	Est	Mean	Est	Mean	Est
	Control		Control		Control		Control		Control		Control	
Enrollment												
Technical Degree	0.457	0.024** (0.012)	0.395	0.087*** (0.026)	0.349	-0.064*** (0.008)	0.344	-0.061*** (0.011)	0.357	-0.042*** (0.014)	0.381	-0.062*** (0.019)
Professional Degree TS	0.166	0.025*** (0.009)	0.191	-0.003 (0.020)	0.192	-0.055*** (0.006)	0.196	-0.058*** (0.009)	0.173	0.008 (0.012)	0.165	0.009 (0.015)
Professional Degree Univ	0.241	-0.029*** (0.009)	0.281	-0.069*** (0.021)	0.408	0.122*** (0.008)	0.407	0.127*** (0.011)	0.407	0.045*** (0.014)	0.390	0.057*** (0.018)
Any Degree	0.864	0.019** (0.008)	0.867	0.015 (0.017)	0.950	0.003 (0.004)	0.948	0.007 (0.005)	0.937	0.011 (0.008)	0.936	0.005 (0.011)
Graduation												
Technical Degree	0.269	0.006 (0.010)	0.203	0.072*** (0.023)	0.256	-0.032*** (0.008)	0.259	-0.033*** (0.010)	0.209	-0.027** (0.012)	0.208	-0.021 (0.016)
Professional Degree TS	0.082	0.018*** (0.007)	0.070	0.031** (0.015)	0.141	-0.036*** (0.006)	0.144	-0.039*** (0.008)	0.089	-0.007 (0.009)	0.084	-0.006 (0.011)
Professional Degree Univ	0.100	-0.005 (0.007)	0.112	-0.018 (0.015)	0.224	0.044*** (0.008)	0.226	0.042*** (0.011)	0.158	-0.009 (0.010)	0.155	-0.009 (0.014)
Any Degree	0.432	0.013 (0.011)	0.370	0.074*** (0.025)	0.582	-0.016* (0.009)	0.589	-0.021* (0.012)	0.434	-0.039*** (0.015)	0.425	-0.032 (0.020)
Actual Costs (At Graduation)												
Total Years	3.520	0.151*** (0.049)	3.449	0.210* (0.107)	4.513	0.124*** (0.035)	4.505	0.145*** (0.047)	4.276	0.197*** (0.062)	4.229	0.203** (0.082)
Total Cost (2017 USD)	6,900	377.689** (157.175)	6,752	572.029* (345.101)	10,697	898.058*** (171.202)	10,694	1,072.296*** (228.095)	10,162	592.163** (253.018)	10,115	745.485** (335.686)
Expected Benefits (At Graduation)												
Expected Income	10,570	22.394 (82.122)	10,503	81.935 (180.301)	12,108	82.424 (92.278)	12,162	-2.203 (122.943)	11,113	-280.584** (119.118)	11,078	-282.568* (158.041)
Expected Employment	0.590	0.000 (0.002)	0.586	0.005 (0.005)	0.628	0.004 (0.002)	0.630	0.002 (0.003)	0.606	-0.007** (0.003)	0.603	-0.008* (0.004)
Benefits-Costs												
PV Benefits-Costs	-1,160	-1,258.343 (1,691.720)	-1,917	-625.068 (3,714.210)	19,118	-245.067 (1,947.323)	20,314	-2,384.450 (2,594.450)	520	-8,357.798*** (2,504.245)	240	-8,534.643** (3,322.531)
Obs	96,719		96,719		100,788		100,788		35,543		35,543	

Table 2.A.2 shows RD estimates using alternative polynomials (of order 2 and 3) of the running variable. Actual Costs (At Graduation) refer to how much students have spent 7 to 9 years after high school graduation. Expected Benefits (At Graduation) refer to expected annual incomes four years after graduation and employment probabilities one year after graduation based on where students have graduated from. I assume an expected annual wage of 8,844 USD and an expected employment rate of 0.55 for students who do not graduate or never enroll in higher education. PV Benefits-Costs are present-discounted expected earnings net of costs, assuming an interest rate of 3.5% and zero wage growth (see Section D for details). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2.A.3: Estimates using Clustered Standard Errors

	Loans TS vs No Loans		Loans TS & U vs Loans TS		Loans TS & U vs No Loans	
	Mean	Est	Mean	Est	Mean	Est
	Control		Control		Control	
Enrollment						
Technical Degree	0.491	-0.007 (0.013)	0.356	-0.076*** (0.006)	0.374	-0.063*** (0.010)
Professional Degree TS	0.146	0.045*** (0.008)	0.197	-0.061*** (0.005)	0.176	0.008 (0.007)
Professional Degree Univ	0.235	-0.026*** (0.005)	0.397	0.144*** (0.006)	0.389	0.066*** (0.012)
Any Degree	0.872	0.013*** (0.002)	0.950	0.006** (0.003)	0.940	0.011** (0.005)
Graduation						
Technical Degree	0.278	0.001 (0.008)	0.255	-0.035*** (0.006)	0.218	-0.031*** (0.008)
Professional Degree TS	0.082	0.021*** (0.003)	0.145	-0.037*** (0.004)	0.091	-0.001 (0.006)
Professional Degree Univ	0.100	-0.003* (0.002)	0.219	0.061*** (0.006)	0.152	0.005 (0.007)
Any Degree	0.436	0.016** (0.007)	0.580	-0.003 (0.008)	0.439	-0.025** (0.011)
Actual Costs (At Graduation)						
Total Years	3.542	0.144*** (0.014)	4.498	0.188*** (0.035)	4.247	0.245*** (0.050)
Total Cost (2017 USD)	6,946	344*** (49)	10,495	1,110*** (334)	9,857	874*** (323)
Expected Benefits (At Graduation)						
Expected Annual Earnings (2017 USD)	10,624	-6 (31)	12,055	145* (79)	11,098	-120 (89)
Expected Employment	0.592	-0.000 (0.001)	0.628	0.005*** (0.002)	0.607	-0.005** (0.002)
Benefits-Costs						
PV Benefits-Costs	-208	-1,811*** (527)	18,271	321 (1,646)	738	-5,613*** (1,889)
Obs	96,719		100,788		35,543	

Table 2.A.3 shows RD estimates clustering the standard errors by the running variable. Actual Costs (At Graduation) refer to how much students have spent 7 to 9 years after high school graduation. Expected Benefits (At Graduation) refer to expected annual incomes four years after graduation and employment probabilities one year after graduation based on where students have graduated from. I assume an expected annual wage of 8,844 USD and an expected employment rate of 0.55 for students who do not graduate or never enroll in higher education. PV Benefits-Costs are present-discounted expected earnings net of costs, assuming an interest rate of 3.5% and zero wage growth (see Section D for details). *** p < 0.01, ** p < 0.05, * p < 0.10.

2.B Density Tests

Figure 2.B.1: Density Tests

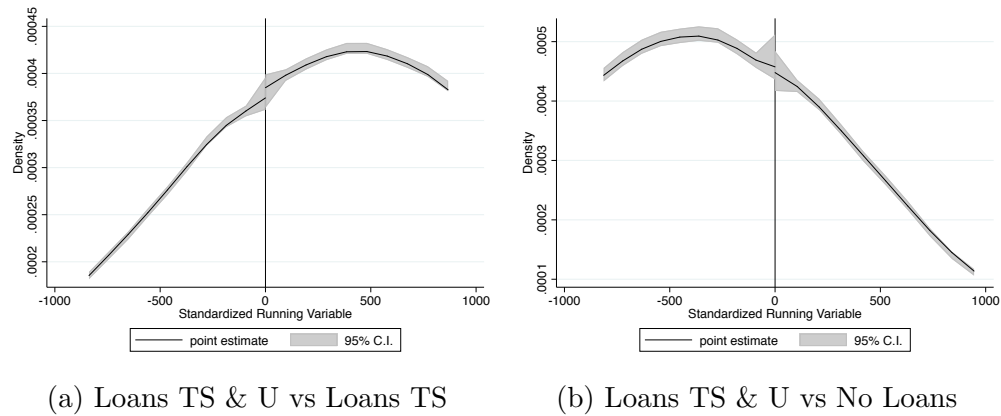


Figure 2.B.1 shows plots from `rddensity` ([Cattaneo et al. \(2016b\)](#))

2.C Sensitivity Analysis

Table 2.C.1: Effect of Loan Access on Expected Benefits-Costs (Sensitivity Analysis)

		Loans TS vs No Loans		Loans TS & U vs Loans TS		Loans TS & U vs No Loans	
		Mean Control	Est	Mean Control	Est	Mean Control	Est
Interest Rate							
3.5%	-208		-1,956** (923)	18,271	229 (1,302)	738	-5,703*** (1,683)
4.0%	-2,989		-1,942** (857)	13,254	18 (1,209)	-2,784	-5,527*** (1,564)
6.0%	-11,438		-1,897*** (662)	-1,986	-626 (928)	-13,484	-4,991*** (1,210)
Wage Growth							
0%	-208		-1,956** (923)	18,271	229 (1,302)	738	-5,703*** (1,683)
1.5%	9,262		-1,939* (1,118)	34,628	916 (1,577)	12,641	-6,116*** (2,034)
2%	13,043		-1,937 (1,200)	41,216	1,193 (1,691)	17,401	-6,295*** (2,181)
Obs	95,515			99,570		35,163	

Table 2.C.1 shows alternative RD estimates of present-discounted expected earnings net of costs assuming different interest rates and wage growth rates. Estimates with varying interest rate all assume a wage growth of zero. Estimates with varying wage growth rate assume an interest rate of 3.5% (see Section D for details). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

2.D Balance Checks for Sub-Groups

Table 2.D.1: Balance for Students of Varying Test Score on the Margin of Getting Access to Loans TS vs no Loans

	≤ 381		381-433		> 433	
	Mean Control	Est	Mean Control	Est	Mean Control	Est
Socioeconomic Characteristics						
Female	0.659	-0.012 (0.011)	0.611	0.008 (0.011)	0.569	-0.009 (0.011)
Age	18.359	0.087 (0.062)	18.100	0.001 (0.046)	17.957	0.011 (0.033)
Lives in the capital	0.237	0.020** (0.010)	0.293	0.004 (0.010)	0.333	-0.001 (0.011)
Public School	0.620	-0.012 (0.011)	0.511	0.012 (0.011)	0.424	0.023** (0.011)
Private Voucher School	0.376	0.012 (0.011)	0.484	-0.016 (0.011)	0.570	-0.030*** (0.011)
Private School	0.004	0.000 (0.002)	0.004	0.003 (0.002)	0.005	0.007** (0.003)
Total HH members	4.504	0.051 (0.041)	4.522	-0.002 (0.041)	4.525	-0.010 (0.040)
Total HH members work	1.140	0.021 (0.016)	1.164	0.018 (0.016)	1.188	-0.006 (0.016)
Head of HH father	0.581	-0.009 (0.011)	0.565	0.034*** (0.011)	0.604	-0.007 (0.012)
Head of HH mother	0.285	0.001 (0.010)	0.320	-0.026** (0.011)	0.296	0.004 (0.011)
Annual Income (2017 USD)	4,250	197.739** (78.519)	5,130	136.657 (91.125)	6,228	-186.084* (107.744)
Mother primary ed	0.390	0.002 (0.011)	0.284	0.002 (0.010)	0.195	0.019** (0.009)
Mother secondary ed	0.531	-0.002 (0.011)	0.599	0.004 (0.011)	0.643	-0.022** (0.011)
Mother tertiary ed	0.078	-0.000 (0.006)	0.117	-0.006 (0.007)	0.162	0.004 (0.009)
Father primary ed	0.379	0.010 (0.011)	0.283	-0.012 (0.010)	0.182	0.034*** (0.009)
Father secondary ed	0.492	-0.011 (0.012)	0.550	0.009 (0.012)	0.595	-0.011 (0.012)
Father tertiary ed	0.129	0.001 (0.008)	0.167	0.003 (0.009)	0.223	-0.023** (0.010)
Father works full-time	0.523	0.019 (0.012)	0.582	0.001 (0.012)	0.607	-0.004 (0.012)
Mother works full-time	0.239	0.003 (0.010)	0.287	-0.004 (0.010)	0.297	0.021* (0.011)
Obs	32,142		31,733		32,844	

Table 2.D.1 examines whether individuals just above and just below the cutoff in the subsamples analyzed are similar in terms of their observable characteristics. *** p < 0.01, ** p < 0.05, * p < 0.10.

Table 2.D.2: Balance for Students of Varying GPA on the Margin of Getting Access to Loans TS & U vs Loans TS

	≤ 5.5		5.5-5.8		> 5.8	
	Mean Control	Est	Mean Control	Est	Mean Control	Est
Socioeconomic Characteristics						
Female	0.557	0.008 (0.010)	0.635	-0.015 (0.011)	0.681	0.010 (0.010)
Age	17.876	0.045 (0.031)	17.818	-0.002 (0.034)	17.870	0.087 (0.059)
Lives in the capital	0.342	-0.008 (0.010)	0.286	-0.002 (0.010)	0.245	0.000 (0.009)
Public School	0.428	-0.018* (0.010)	0.497	0.019* (0.011)	0.606	0.000 (0.011)
Private Voucher School	0.559	0.021** (0.010)	0.496	-0.016 (0.011)	0.390	0.000 (0.011)
Private School	0.013	-0.003 (0.002)	0.007	-0.003 (0.002)	0.004	-0.001 (0.001)
Total HH members	4.542	-0.072** (0.036)	4.525	0.008 (0.039)	4.485	0.031 (0.037)
Total HH members work	1.191	-0.001 (0.015)	1.148	0.012 (0.015)	1.073	0.006 (0.014)
Head of HH father	0.606	-0.014 (0.010)	0.611	-0.007 (0.011)	0.622	0.001 (0.011)
Head of HH mother	0.293	0.017* (0.010)	0.288	0.009 (0.011)	0.266	0.002 (0.010)
Annual Income (2017 USD)	6,348	-67.129 (98.207)	5,673	29.622 (98.640)	4,805	15.709 (84.612)
Mother primary ed	0.197	-0.009 (0.008)	0.250	-0.005 (0.010)	0.347	-0.002 (0.011)
Mother secondary ed	0.622	0.007 (0.010)	0.605	-0.004 (0.011)	0.559	0.001 (0.011)
Mother tertiary ed	0.182	0.002 (0.008)	0.145	0.009 (0.008)	0.095	0.001 (0.007)
Father primary ed	0.201	-0.021** (0.009)	0.233	0.018* (0.010)	0.363	-0.010 (0.011)
Father secondary ed	0.587	-0.001 (0.011)	0.567	-0.015 (0.012)	0.496	0.009 (0.011)
Father tertiary ed	0.212	0.022** (0.009)	0.200	-0.003 (0.010)	0.140	0.000 (0.008)
Father works full-time	0.602	0.010 (0.011)	0.569	0.023* (0.012)	0.538	-0.009 (0.012)
Mother works full-time	0.324	0.011 (0.010)	0.290	0.005 (0.011)	0.233	0.004 (0.009)
Obs	35,159		30,051		35,578	

Table 2.D.2 examines whether individuals just above and just below the cutoff in the subsamples analyzed are similar in terms of their observable characteristics. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

3.A Proof of Proposition 1

The IV estimator of β in regression (3.1) using Z as an instrument is equivalent to the Wald ratio, that is:

$$\beta_{IV} = \frac{E[y_x|Z=1] - E[y_x|Z=0]}{E[d_x|Z=1] - E[d_x|Z=0]} = \frac{E[y_x(1) - y_x(0)]}{E[d_x(1) - d_x(0)]},$$

where the second equality follows from Assumption 1 (independence), and Assumption 2 (relevance) ensures that it is well defined. Under Assumption 4 (monotonicity), we can write the denominator as:

$$E[d_x(1) - d_x(0)] = Pr(d_x(1) = 1 \wedge d_x(0) = 0) = Pr(\Delta d_x = 1) = \omega$$

With Assumption 3 (exclusion restriction), we can write the numerator as:

$$E[y_x(1) - y_x(0)] = E[q'_x \Delta d]$$

From assumptions 4 (monotonicity) and 5 (IIA), we know that either $q'_x \Delta d = 0$ or $q'_x \Delta d = q_{xx} - q_{xk}$ for some $k \neq x$. Using this, and the law of iterated expectations, we obtain:

$$\begin{aligned} E[y_x(1) - y_x(0)] &= \sum_{\forall k \in J - \{x\}} E[q_{xx} - q_{xk} | \Delta d_x = 1, \Delta d_k = -1] \cdot Pr(\Delta d_x = 1, \Delta d_k = -1) \\ &= \sum_{\forall k \in J - \{x\}} E[q_{xx} - q_{xk} | \Delta d_x = 1, \Delta d_k = -1] \cdot \omega_k, \end{aligned}$$

and so equation (3.2) follows.

3.B Proof of Proposition 2

We follow the same logic than the proof of Proposition 1. From assumptions 1 and 2, we can write β_{IV}^c as:

$$\beta_{IV}^c = \frac{E[y_x^c(1) - y_x^c(0)]}{E[d_x^c(1) - d_x^c(0)]}$$

From assumptions 4 and 5, we know that either $\Delta d_x^c = 1$ or $\Delta d_x^c = 0$ and thus the denominator is:

$$E[d_x^c(1) - d_x^c(0)] = Pr(d_x^c(1) = 1 \wedge d_x^c(0) = 0) = Pr(\Delta d_x^c = 1) = \omega_c$$

From assumption 3 (exclusion restriction), we can write the numerator as:

$$E[y_x^c(1) - y_x^c(0)] = E[q_x^c \Delta d],$$

where $q_x^c = (q_{x1}^c, \dots, q_{xJ}^c)'$. We know from assumptions 4 and 5, that either $q_x^c \Delta d = 0$ or $q_x^c \Delta d = q_{xx}^c - q_{xk}^c$ for some $k \neq x$. Using this and the law of iterated expectations, we get:

$$\begin{aligned} E[y_x^c(1) - y_x^c(0)] &= \sum_{\forall k \in J - \{x\}} E[q_{xx}^c - q_{xk}^c | \Delta d_x = 1, \Delta d_k = -1] \cdot Pr(\Delta d_x = 1, \Delta d_k = -1) \\ &= \sum_{\forall k \in J - \{x\}} E[q_{xx}^c - q_{xk}^c | \Delta d_x = 1, \Delta d_k = -1] \cdot \omega_k, \end{aligned}$$

The term inside the expectation can be written as $(q_{xx}^c - q_{xk}^c) \cdot c_k + (q_{xx}^c - q_{xk}^c) \cdot (1 - c_k)$, and from Assumption 6, the first part equals zero. We then arrive at:

$$E[y_x^c(1) - y_x^c(0)] = \sum_{\forall k \in J - \{x\}} E[(q_{xx}^c - q_{xk}^c)(1 - c_k) | \Delta d_x = 1, \Delta d_k = -1] \cdot \omega_k,$$

from which (3.4) follows.